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Judicial Elections, Public Opinion, and their Impact on State Criminal Justice Policy

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JUDICIAL ELECTIONS, PUBLIC OPINION, AND THEIR IMPACT ON STATE CRIMINAL JUSTICE POLICY

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the College of Arts and Sciences at the University of Kentucky

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ABSTRACT OF DISSERTATION

JUDICIAL ELECTIONS, PUBLIC OPINION,
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CRIMINAL JUSTICE POLICY

This dissertation explores whether and how the re-election prospects faced by trial court judges in many American states influence criminal justice policy, specifically, state levels of incarceration, as well as the disparity in rates of incarceration for Whites and Blacks. Do states where trial court judges must worry about facing reelection tend to encourage judicial behavior that results in higher incarceration rates? And are levels of incarceration and racial disparities in the states influenced by the proportion of the state publics who want more punitive policies? These are clearly important questions because they speak directly to several normative and empirical issues concerning institutional design, representation, and equal treatment under the law. In addition to the role that court retention procedures are found to play, the dissertation also investigates the influence of a range of other factors, especially state characteristics, that shape state incarceration rates and racial disparities in punishment.

To answer these questions, I first construct a theory of judicial retention goals that leads to testable hypotheses about how institutional design and state public opinion influence incarceration rates and racial disparity in state prison populations. I also assemble an original dataset by drawing information from a variety of sources. Annual state-level incarceration rates, my principal dependent variable, is drawn from the National Prisoner Statistics (NPS), 1978-2015 data compiled annually by the Department of Justice. A primary independent variable, judicial retention procedures is derived from the National Center for State Courts. Population data come from the U.S. Census Bureau. An original dataset of yearly state-level public punitiveness provides more nuanced data for exploring the linkages between election goals and judicial behavior. Finally, hypotheses are tested with a variety of methods, including multilevel regression and post-stratification (MRP), recursive analysis, time-series analysis, and difference-in-differences estimation.

Before discussing how this dissertation contributes to the scholarly literature, it is important to note that this study does not attempt to resolve the longstanding argument between two values of judicial decision-making—judicial accountability versus and judicial independence—that continues to vex the judicial literature. A strong normative case for an independent judiciary can be made. American courts are considered a

countermajoritarian institution, guarding against the tyranny of the majority and protecting the rights of minorities from the whims of the masses. Jurists, in an ideal world, would rule in strict accordance with the law, except when the law was used as an instrument of injustice. Such utopian judges—dispensing justice free of extra-legal considerations—would not, of course, consider the opinions of the public when making rulings. They would not be more punitive simply because the public wants them to be. They would not dispense "justice" to members of one group of citizens more harshly—or differently at all—than to members of another group. Indeed, such judicial independence would ideally put a brake on the excesses of punitive policies, which have produced questionable results in deterring crime.

However, in the American judiciary, such a judge is relegated to mythical status. A robust literature in political science demonstrates that judges who staff American courts are not jurisprudential robots. Judges are influenced—whether explicitly or implicitly—by myriad extra-legal considerations including their policy preferences, the anticipated behavior of other actors in the politico-legal system, or even how recently they have taken a break. These influences, however, are to be considered seriously by political scientists and other scholars of the courts. More to the point, if judges are or will be influenced by political matters regardless of whether or not they *should* be, the question becomes one of accountability. Put another way, if judges are political actors, to whom should they be politically responsible?

Most scholars in the state courts literature argue that judges are held accountable by the voters. If voters are to exercise such a role, jurists must take into account the preferences of the electorate and respond strategically to those preferences while still attempting to maximize their own preferences, whether those be policy-related or motivated by jurisprudence. Elections, thus, provide political accountability to a political actor who frequently behaves is a very political way.

But evermore responsiveness to public opinion is not necessarily always desirable and is sometimes certainly very undesirable. To be certain, mass incarceration is accompanied by a host of negative consequences. However, judicial elections also provide a solution to these problems. Judges who go rogue in states (or the federal judiciary) with strong institutional tenure protections are unlikely to face consequences for their wayward behavior. On the contrary, judges who must stand for re-election are very likely to face consequences for straying too far. It is even possible for a vocal minority to sufficiently mobilize against and ultimately depose a rogue judge, especially considering the ordinarily low-profile nature of judicial elections.

Over the course of this dissertation, I accomplish four major scholarly endeavors. First, I develop a democratic theory of state trial court judicial behavior. The theory is built on the punitiveness of public opinion and the salience of crime. From those foundational elements, I theorize that elected judges are responsive to public opinion on criminal justice issues in an effort to secure re-election. From this theory, I hypothesize that incarceration rates and racial disparity in prison populations will be higher in states with elected judges than in non-electoral states and that public opinion will condition the effect of elections such that as public opinion becomes more punitive in electoral states, the levels of incarceration and racial disparity will rise, compared to non-electoral states.

Second, I assemble and validate an original dataset of public opinion toward crime and punishment. The first of its kind, this dataset, which is introduced in chapter 3, varies

both over-time—year to year—and across space—state to state. Using the data compiled from more than 30 years of public opinion surveys, I demonstrate its validity by replicating previous work on the impact public opinion has on incarceration rates. I further discuss the utility of the data for myriad social scientific questions, both in political science and in other disciplines.

Third, I explore the impact of judicial elections on incarceration rates and how the impact of those institutions on the penal system are conditioned by public opinion. Specifically, in chapter 4, I show that, when contrasted to states where judges are retained by some mechanism other than elections, states that use elections to keep judges on the bench are more responsive to public opinion and that incarceration rates are higher in electoral states when the public desires a more controlling response to crime. Additionally, I show that non-partisan elections create the conditions for judges to be more responsive to public opinion than partisan elections. Despite the finding from the time-series analysis that non-partisan elections are associated with more punitive outcomes, a causal analysis shows that none of the three states that changed from partisan to non-partisan elections experienced an increase in incarceration rates following the reform that would not have existed absent the reform.

Finally, I show that racial disparities in incarceration are largely unaffected by the institutional arrangements for judicial retention. Time-series models indicate that, when compared to states without judicial elections, states with them are statistically no different in terms of the disparity between Black and White prison populations. However, when comparing types of elections, non-partisan elections produce Black disparity that is more responsive to public opinion than partisan elections.

The project contributes to research in several fields of study. First, I extend the argument that that judicial elections provide voters with a mechanism for accountability to an important but often neglected setting of state trial court judicial decision-making. Trial court accountability to public opinion is important for government's response to citizen demands for public safety. Prior research on the effects of judicial elections has focused primarily on appellate courts, with much less systematic research on the "workhorse" of the court system at the state level. This project moves beyond the study of appellate courts to study trial courts, expanding our understanding of previous findings. Second, I contribute to the growing literature that examines the political, partisan, and ideological antecedents of the criminal justice system. Previous research shows that electoral considerations affect criminal justice policy and that the partisan nature of the political system influences incarceration rates. I extend these findings to study state judiciaries. Third, I contribute to the fields of public opinion and state politics by demonstrating that elected trial state judges can improve representation by being responsive to the policy preferences of the public in the area of criminal justice policies.

KEYWORDS: Public Opinion, Judicial Elections, State Politics, Criminal Justice, Incarceration, Racial Disparities

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I began the Acknowledgements section of my master's thesis by thanking God for his guidance. In that 2014 project, I wrote, "If not for my Creator, I would not have had the strength or wisdom to complete this project" (shameless self-citation omitted). That statement is infinitely truer today and about this project than it was some six years ago. My first praise and thanks is to Him.

Writing a dissertation is, in many ways, like raising a child. You conceive, birth, defend, nurture, and guide a child. In similar ways, one brings a research idea into the world, develops, nurtures, directs, and, ultimately, defends a dissertation. Many times along both paths, one grows frustrated or even angry, considers throwing in the towel. In those instances, you walk away and collect your thoughts, then come back. As many times, hopefully *more* times, one is filled with joy, hope, and adoration for how their "project" is coming along. One final way that the two life events are similar: it takes a village. This section acknowledges, recognizes, and extends my deepest gratitude to my village.

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This is my village. These are the people who have helped this project become what it is today. And, once again, like raising a child, at the end of the day, the village has had their say and done their part. The ultimate responsibility lies with the parent. My village did their part. Any remaining mistakes are completely and utterly my own.

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Chapter 1: Introduction

In 1995, Mark Ciavarella ran a campaign to be elected as a trial judge in Luzerne County, Pennsylvania. In one of his now infamous campaign television advertisements, Ciavarella walked toward the camera and said the following: "If you are…convicted of murder or rape, or violent crimes against our children or the elderly, you can expect that I will impose the maximum sentence allowed by law."[1] This promise is a premier example of the kind of tough-on-crime rhetoric that many elected officials employ for political gain. And it raises an important question. When judges are elected to take a seat on the bench and re-elected to retain that seat, are they tougher on crime than their counterparts who do not face electoral considerations?

The United States justice system incarcerates more convicted criminals than any other system in the world. Over the course of just a few decades, the number of incarcerated Americans grew dramatically, increasing rapidly with the onset of the war on drugs in the 1970s and growing exponentially during the 1980s. According to the non-profit organization, The Sentencing Project, more than two million Americans are currently housed in the nation's prisons and jails, a five-fold increase since 1980 (The Sentencing Project 2020). Moreover, racial minorities have experienced a disproportionate impact of the carceral system. While one in 17 White men and one in 111 White women are likely to be incarcerated during their lifetimes, the likelihood is much higher—one in three Black men and one in 18 Black women—for Black Americans (The Sentencing Project 2020).

Scholars have long sought to understand the factors driving mass incarceration and racial disparities in the US. Previous research has explored the criminological (e.g.,

[1] https://youtu.be/YrssaBxckNc?t=43

Beichner and Spohn 2005; Chambliss and Seidman 1971; Spohn 2000), sociological (e.g., Allport 1958; Cottrell et al. 2019; Spohn and Cederblom 1991; Spohn et al. 1981; Zatz 1987), economic (e.g., Hooks et al. 2004; Rosenfeld 2009; Rosenfeld and Fornango 2007; Stuntz 1998), and political (e.g., Enns 2014; Enns and Ramirez 2018; Johnson 2001; Yates 1997; Yates and Fording 2005) antecedents of punitive criminal justice policies in the United States.

Only a handful of studies have explored the impact of judicial elections on sentencing, incarceration, and racial disparities, however (e.g., Brooks and Raphael 2002; Canes-Wrone et al. 2014; Cohen and Yang 2018; Huber and Gordon 2004; Park 2017). And even if we cast a wider net to include studies of how judicial elections can influence punitive policies in the US, such research is limited in several respects. First, much of the judicial elections literature has focused on state appellate courts, especially state supreme courts (e.g., Brace and Boyea 2008; Brace and Hall 1995; Caldarone et al. 2009; Canes-Wrone et al. 2014; Canes-Wrone et al. 2012). Relatively fewer studies examine the policy implications of trial court elections (but, see, e.g., Cohen and Yang 2018; Gordon and Huber 2007; Huber and Gordon 2004; Park 2017; Rachlinski et al. 2009). Such a disproportionate focus on appellate courts is problematic given the unique position of trial courts as the hearers of fact: the vast majority of cases in the United States originate in state trial courts, so most criminal defendants serving prison time are processed through these courts.

Second, even the studies exploring policy consequences at the trial court level have been limited geographically, focusing on just one or a few states or localities. For example, Huber and Gordon (2004) investigate the influence of electoral proximity on sentencing

decision in Pennsylvania, while Gordon and Huber (2007) and Park (2017) take advantage of the natural experiment in Kansas' trial court districts to explore the impact of elections on sentencing and racial disparities, respectively.

Such limitations raise important questions of generalizability. Because appellate judges face different constraints and incentives than trial judges, results from studies of supervising courts may not generalize to lower courts, even to those within the same state. Moreover, in the US federalist system—where states operate largely autonomously from one another—single-state studies, while advantageous in some respects, are unlikely to produce findings that are generalizable to other jurisdictions. Thus, the question remains: do trial court elections increase punitiveness in the states? The current research offers the first multi-state study of the effect of trial court elections on criminal justice policy outcomes.

If elections are important, it is also essential to understand the role of public opinion in driving criminal justice policies (and trial court outcomes) in the states. Previous research shows that public opinion is a critical predictor of policy in many domains at the local, state, and national levels, including government spending (Soroka and Wlezien 2010; Tausanovitch and Warshaw 2014), education (Houston 2019), and gay rights (Lax and Phillips 2009). Importantly, scholarship also shows that criminal justice policy is shaped by public opinion in areas such as incarceration (Enns 2014), the death penalty (Lax and Phillips 2012), and prison privatization (Enns and Ramirez 2018). Thus, if judicial elections help drive punitive outcomes, mass support is likely to be an important contributing factor.

The remainder of this chapter proceeds as follows. I begin with a discussion of elite and policy responsiveness to public opinion. I then discuss the history of judicial elections in the states and briefly review the social sciences research on judicial elections. I then propose an analytical framework through which to investigate the role of public opinion and judicial elections in shaping criminal justice policy. I discuss some analytical considerations for this work before laying out a roadmap for the remainder of the chapters in this work.

Elite and Policy Responsiveness to Public Opinion

In this section, I explore research that connects public opinion to policy outcomes. It is an old trope that elected officials do not listen to the people. This trope is passed on in the public discourse as conventional wisdom. Each new recipient of this message believes it and, like a game of telephone, passes it on to the next unsuspecting player. But is it true? Do elites ignore the will of the people?[2] Or, do elites actually translate the preferences of the people into policy? The truth is that policy representation is far more complicated than these extremes suggest.

Although some scholars argue that policy is more responsive to the economic elite (Bartels 2008b; but, see Lax et al. 2019), scores of studies have shown that policies reflect the preferences of voters at all levels of government in the United States. Consider, for example, at the city level, the level of conservatism in a city's population predicts a significantly smaller level of spending, significantly lower taxes, and a greater share of revenue coming from sales (rather than income) tax (Tausanovitch and Warshaw 2014).

[2] I proceed by setting aside, for the sake of argument, the idea that the people may not even have a will (Converse 1964; Zaller 1992) and assume that they do, at least in the aggregate (Erikson et al. 2002a), and that that will can be accurately measured.

At the national level, policies such as spending (Soroka and Wlezien 2010) and incarceration rates (Enns 2014) are directly responsive to the public's expressed policy preferences (see, also, Erikson et al. 2002a, 2002b).

Evidence for policy responsiveness to public opinion is even greater in the states. For example, Erikson et al. (1993) showed strong correlations between ideological identification of state residents and an index measure of policy liberalism, composed of eight policy areas.[3] More recently, a series of studies has demonstrated that state policy in specific domains is responsive to the policy preferences of the public on that given topic, including gay rights (Lax and Phillips 2009), incarceration (Enns 2016), prison privatization (Enns and Ramirez 2018), agenda-setting (Barberá et al. 2019), and education spending (Houston 2019) just to name a few (see, also, Lax and Phillips 2012; Pickett 2019; Tausanovitch 2019).

Of course, most jurisdictions in the United States are not pure democracies. Thus, the people do not directly vote on most policies.[4] Still, policy is enacted that reflects the preferences of the public, an outcome that is accomplished through party competition (Erikson et al. 1993) in elections (Ginsberg 1976). Candidates for public office campaign on issues that the public cares about—i.e., issues that are salient. The public then selects

[3] Those eight policy areas are: Education spending, Medicaid eligibility, AFDC (welfare) eligibility, consumer protection, criminal justice, and gambling legalization (Erikson et al. 1993, 75). The bivariate correlations and other statistical analyses (OLS and 2SLS) presented in Erikson et al. (1993) are from data that are pooled across more than a decade of data. Their evidence does not depend on time-series analysis because measures of state ideology are stationary across time (Erikson et al. 1993, 94).

[4] By "most policies" I acknowledge that many states also allow for more direct forms of democracy via the ballot initiative process, whereby policies are initially approved by the public. Still, the number of ballot initiatives is vastly overshadowed by the number of legislative bills, executive orders, bureaucratic rules and regulations, and judicial decisions that are enacted or implemented.

the candidate who is closest to their ideal point on those issues (Lau and Redlawsk 1997; Lau et al. 2008). Once elected, those public officials then translate their preferences—preferences that match those of the public—into policy.[5]

The question I raise here is whether this same dynamic operates for trial judges in the realm of criminal justice policy? In other words, do elected judges run on a given platform, do voters select the judicial candidate closest to their ideal point, and do judges then translate the public's preferences into policy in the criminal justice arena with relatively more punitive decisions? Before discussing my expectations about this dynamic, I explore the concept of judicial selection in the states.

Judicial Selection in the States

In the sections that follow, I discuss judicial selection in the states. I begin with an overview of the ideals that animate the debate between election proponents and election opponents—accountability versus independence. I then provide a brief historical accounting of the reforms made to judicial selection over the years in the states, showing that the debate predates the nation's founding and how the terms of the debate have shifted in the various rounds. Following the history of reforms, I extrapolate the various types of elections present in the modern era and review some of the existing research on the impacts of judicial elections.

[5] The notion that elected officials behave in accordance with the preferences of the electorate fits with the delegate model of representation, contrary to the trustee model in which voters select officials who are entrusted to satisfactorily perform their duties with little influence of public opinion.

Accountability versus Independence

When thinking about the design of the judiciary, an important question that needs to be asked is, to whom, if anyone, should judges be accountable? Should they answer to other democratic institutions in a separation of powers arrangement? As government officials in a democratic society, should they be accountable to the public? Or should they answer only to other judges in a judicial hierarchy?

The answers to these question lead to a clearer understanding of how judges should be selected and, more importantly for the question of accountability, retained. Judicial selection and retention can manifest in two general ways to secure the preferred nature of judicial accountability. The first mechanism provides some kind of accountability to an actor outside the judicial hierarchy. Popular election and re-election creates such accountability for judges. Likewise, accountability exists in states where judges are subject to election and retention votes by state legislators. Judicial elections are quite prominent in the American states. Worldwide, however, very few countries select or retain judges via popular election, making judicial elections an "American innovation" (Friedman 2009).

On the other hand, elite appointment and lifetime tenure, mirroring the American federal system of judicial selection, provide judges with some degree of independence, i.e., the institutional protections to insulate them from extra-judicial pressures, such as public opinion. Such a system makes judges accountable only to the judicial hierarchy through the system of review and reversal by an appellate court. Technically, of course, federal judges, once on the bench, are subject to the threat of impeachment and thus accountable to the Congress, but that tool has been infrequently used in American history. Thus, appointed and tenured judges are theoretically protected from accountability to sources

outside the judicial system. And even accountability to the judicial hierarchy does not threaten the job of a judge, so appointment systems do not provide meaningful accountability, particularly to the public.

To whom, if anyone, should judges be accountable? To the other branches of government? To the people directly? Or to no one except the law and the judicial hierarchy? These are the questions that motivate the question of how we select judges. The central tenets of the selection debate are accountability and independence (Geyh 2019; Goelzhauser 2016; Shugerman 2012). These terms though raise the questions: Independent from whom? and Accountability to whom? Shugerman (2012) differentiates between absolute judicial independence and relative judicial independence. In the former, Shugerman argues, judges are fully protected from outside influence and free to rule on the facts of the case and the law. This absolute independence is the preferred normative state of the world for legal reformers and is accomplished, in Shugerman's view, by institutional protections such as life tenure.

Relative independence, on the other hand, is fluid and speaks to the question raised above: Independence from whom? Independence in the colonial and founding sense meant independence from the monarch or executive, but in many states, this meant less independence from the legislature. Independence in the contemporary sense is largely associated with independence from the people, but such freedom also means less independence from political elites. In short, relative independence from one actor leads to less independence from another actor. For the purposes of this research, I define independence as the ability of a judge to rule on the law and the facts of a case without pressure from outside sources and without fear of or concern about the personal

ramifications of their decisions, such as the loss of their judicial career and with it their livelihood.

Defining accountability raises a similar set of concerns. Just as the concept of relative independence requires a tradeoff—independence from one body or institution becomes a lack of independence from another body. Accountability is also relative. As judges become more accountable to one actor, they become less accountable to other actors. Early states granted the selection of judges to state legislatures. Two states, South Carolina and Virginia, still require judges to be reappointed by their legislative bodies. Accountability to the legislature reduces accountability to the executive. When states reformed their judiciaries to permit direct popular election, judges became accountable to the people while becoming less accountable to other elites. Because this project is a work about the impact on public policy of judicial elections, my definition of accountability must reflect that purpose. Thus, accountability is the responsibility of judges to discharge the duties of their office in a way that will curry favor with the voting public. In other words, judges are accountable to the public and thus should be responsive to public opinion.

Finally, independence and accountability are mutually exclusive concepts. They are opposite poles of a judicial magnet. When a judge is accountable to some actor in the system, say the legislature, the same judge is in no way independent from that actor (Gray 2017). Conversely, when a judge is independent of an actor, the judge is not accountable to them in any meaningful way—Shugerman (2012)'s concept of absolute independence.

A Brief History

So, how has this debate between judicial independence and judicial accountability played out historically in the US—especially in the states? Calls for judicial independence

in the United States are older than the nation itself. Among the long list of grievances against King George III aired in the Declaration of Independence was that "He has made Judges dependent on his Will alone, for the tenure of their offices, and the amount and payment of their salaries." Clamoring for a departure from a judiciary controlled by the executive, the founders, in the US Constitution prescribed the appointment of federal judges by the president, subject to confirmation by the Senate. Moreover, Article III of the Constitution grants institutional protection of absolute independence to federal judges by securing both their tenure ("Judges…shall hold their Offices during good Behaviour") and salaries ("Judges…shall, at stated Times, receive for their Services, a Compensation, which shall not be diminished during their Continuance in Office").

During the founding era, states largely followed the example of elite appointment, but some states used minor variations from the federal model (National Center for State Courts 2018). Some states followed the federal model precisely: New York and Massachusetts, for example, set up state judiciaries with judges appointed to life terms by the governor subject to confirmation by the state legislature. Other states, like Florida and North Carolina, established legislative elections for state judges.

Things began to change in the decades preceding the Civil War, however, as states began to adopt the procedure of selecting judges via popular election. Certainly, popular elections shifted judicial accountability from elites (or none at all) to make judges accountable to the populace. Ironically, the argument for judicial accountability historically has been couched in terms of independence (Shugerman 2012). Rather than explicitly claiming that judges should be accountable to the public, election proponents in the early to mid 19th century called for judicial independence from the political elites. Georgia made

the first move in 1812 away from elite appointment to elections. By the start of the Civil War in 1861, popular election was used to select at least some of the judges in 25 states. During reconstruction, some states returned to appointive systems; the federal government imposed judicial appointments in most but not all of the southern states; while voters in other states, like New York and Maryland, which had instituted election pre-Civil War, resoundingly rejected a return to elite appointments (National Center for State Courts 2018; Shugerman 2012).

However, growing concerns about a politicized judiciary, concern about rising crime rates, and the Supreme Court's repeated rebuffing of New Deal legislation served as motivation for another round of change in the early 20th century (Shugerman 2012). Advocates of all political stripes called for reform. Reformers on the left—especially those in the Progressive Movement and the legal community, including the American Bar Association—began to advocate for the dispatch of judicial elections completely. At the very least, the left sought a change from partisan elections to non-partisan contests. Republicans, for their part, concocted the rise of what is today known as the "merit system," or "Missouri Plan" (despite having gotten its initial start in California and its "merit" nomenclature in Nebraska) in which a commission of experts is involved in the judicial selection process, supplying or agreeing to a list of qualified candidates who are then appointed by the governor, serve for a period of time, then face periodic uncontested retention elections. For a time in the 1930s though the 1950s, the merit plan struggled to gain traction. By the end of the 1950s, only five states had adopted a merit system. However, the reform movement was quite successful in the 1960s and 1970s. By 1980,

another 12 states had adopted the merit plan. However, recent reform efforts have failed once again to gain traction.

Since the beginning of the 1980s, only a handful of states have reformed their judicial selection procedures in any meaningful way. Five states—Arkansas, Georgia, Mississippi, North Carolina, and West Virginia—changed from partisan elections to non-partisan elections, although North Carolina recently changed back to partisan elections, and Arkansas is considering a similar move. New Mexico created a hybrid system in which judges are initially retained in partisan elections and subsequently in uncontested retention elections. Meanwhile Utah changed from non-partisan elections to uncontested retention elections, but a state senator there recently introduced legislation to move back to contested elections. Aside from these few reforms and proposed reforms, judicial selection and retention in the states has been at a virtual standstill for the past 40 years.

Today, trial judges in 33 states are initially selected to the bench directly by the electorate and are retained in the same manner. In the remaining 17 states, judges are appointed to the bench by the governor, legislature, or an independent commission. Judges in five appointment states must, after a specified period of time on the bench, face a retention election in which the public votes to keep the judge on the bench; these judges do not face an opponent. Thus, judges in 38 states, regardless of the mechanism by which they are initially selected to the bench must face voters in elections to retain their seat on the bench (National Center for State Courts 2018).

Electoral Types

Judges in states where they are retained by election face three different prospects. First, if they run in one of the 11 states with partisan elections, judicial candidates use

partisan labels to signal to voters their position on policy issues and encourage voters to cast a ballot for their co-partisans.[6] Party labels are permitted and used in both electioneering and on the ballot.

Second, in states where non-partisan elections are held, the use of partisan labels by judicial candidates is proscribed in campaigning and the party identification of candidates is not on the ballot. As such, these judges must behave in ways that appeal to voters to provide the voter with a retrospective accounting of their positions on the issues (Fiorina 1981; Key 1966).

Finally, some judges who are appointed by political elites then face voters in retention elections where the candidate's party affiliation is withheld from voters. The key difference between non-partisan elections and retention elections is competition. In non-partisan elections, candidates compete against other candidates, while there are no challengers in retention elections, and voters answer a simple yes-no question about whether the incumbent judge should be retained.[7]

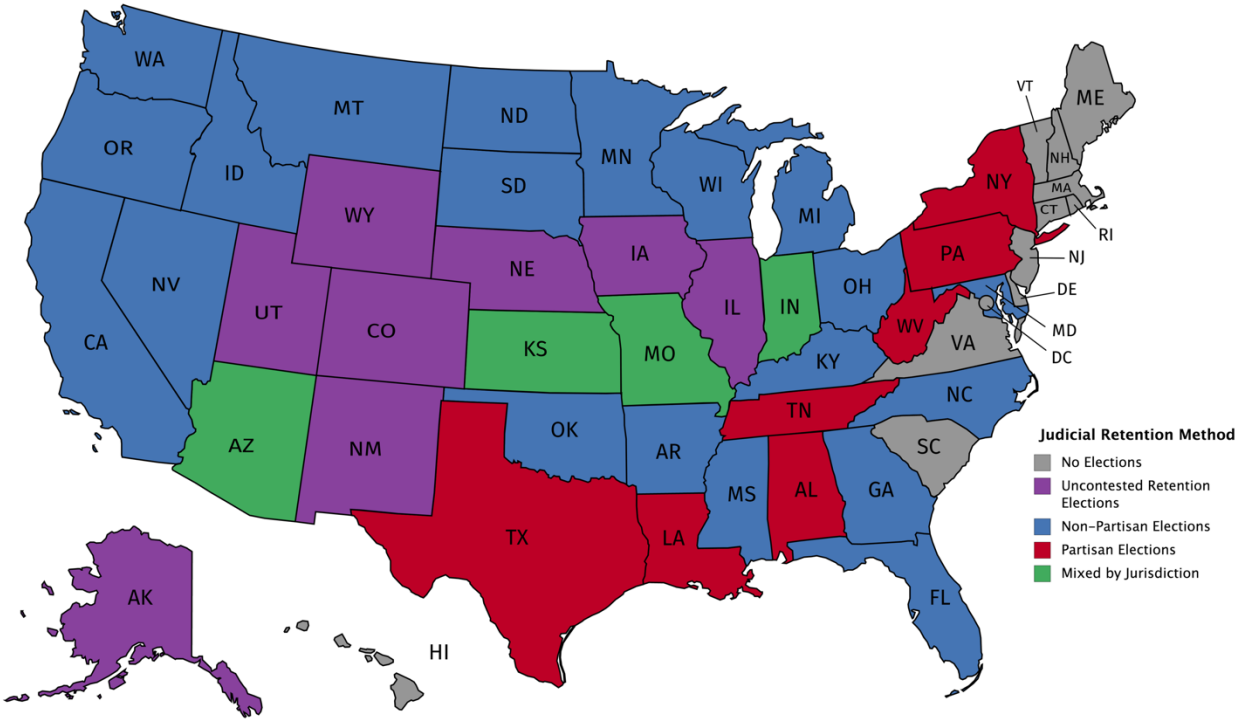
Figure 1-1 displays the mechanism by which trial judges are retained as of 2015. As is clear in figure 1-1, non-partisan elections are the most common mechanism for retaining judges. Even with the 2016 changes in North Carolina and West Virginia, the number of non-partisan states remains today what it was in 2015. Another takeaway from

[6] The 11 states include four states that had partisan elections before a reform. As of the end of the dataset, in 2015, only seven states continued to employ partisan elections for judicial retention.

[7] Since this dissertation investigates the mechanism by which trial judges are retained rather than selected, technically all three types of elections are retention elections. To appropriately distinguish the nominal retention that is part of the "merit system" of judicial selection, I employ the phrase "uncontested retention election" for the remainder of this work.

figure 1-1 is that there are no overarching regional patterns to the retention mechanisms, so any differences in the outcomes observed are unlikely to obtain from regional differences.

Figure 1-1: State Trial Court Retention Mechanisms by State, as of 2015



Note: Judicial selection mechanisms indicated are for the mechanism employed by the state in 2015. Six states–Arkansas, Georgia, Mississippi, New Mexico, North Carolina, and Utah–changed the retention mechanism at various points in the time series under analysis. Four states labeled "Mix by Jurisdiction" employ different selection mechanisms in different jurisdictions, usually at the county or judicial district, within the state. These four states are coded as missing in the dataset. Data are from the National Center for State Courts, available at http://judicialselection.us/judicial_selection/methods/selection_of_judges.cfm?state=. Map created with the free tool at https://mapchart.net/usa.html.

The retention mechanisms displayed in figure 1-1 are not uniform in their constraint on judges. Table 1-1 lists the states with elections, the type of retention election employed, the year the mechanism was adopted, the judges' term length, i.e., the length of time between elections, and whether or not judges can be recalled in the midst of a term. Not only should the election type affect the behavior of judges on the bench, these other features should as well. Greater term lengths should confer additional independence on jurists

(Shugerman 2012). Recall elections could conceivably make judges more responsive to public opinion. So, there are important variations in the electoral environment of state judiciaries.

Table 1-1: State Judicial Electoral Environment Provisions, as of 2015

State	Election Type	Year Enacted	Term Length, in Years	Recall
Alabama	Partisan	1850	6	
Alaska	Uncontested Retention	1959	6	
Arkansas	Non-Partisan	2001	6	
California	Non-Partisan	1911 (1926)	6	Y
Colorado	Uncontested Retention	1966	6	Y
Florida	Non-Partisan	1971	6	
Georgia	Non-Partisan	1983	4	
Idaho	Non-Partisan	1934	4	
Illinois	Uncontested Retention	1964	6	
Iowa	Uncontested Retention	1962	6	
Kentucky	Non-Partisan	1975	8	
Louisiana	Partisan	1852	6	
Maryland	Non-Partisan	1941	15	
Michigan	Non-Partisan*	1939	6	
Minnesota	Non-Partisan	1912	6	Y
Mississippi	Non-Partisan	1994	4	
Montana	Non-Partisan	1935	6	
Nebraska	Uncontested Retention	1962	6	
Nevada	Non-Partisan	1976	6	Y
New Mexico	Uncontested Retention	1988	6	
New York	Partisan	1847	14	
North Carolina	Non-Partisan	2001	8	
North Dakota	Non-Partisan	1909	6	
Ohio	Non-Partisan*	1911	6	
Oklahoma	Non-Partisan	1967	4	
Oregon	Non-Partisan	1931	6	Y
Pennsylvania	Partisan	1921	10	
South Dakota	Non-Partisan	1921 (1972)	8	
Tennessee	Partisan	1853	8	
Texas	Partisan	1876	4	
Utah	Uncontested Retention	1985	6	
Washington	Non-Partisan	1907	4	
West Virginia	Partisan	1862	8	
Wisconsin	Non-Partisan	1848	6	Y
Wyoming	Uncontested Retention	1972	6	

Data derived from National Center for State Courts and Ballotpedia.org as of 2015. Michigan and Ohio have non-partisan general elections, but judges are selected in partisan primaries in Ohio and via a party caucus in Michigan. Where two enactment years are listed, the first is a legislative enactment, and the second, parenthetical, year is the enactment of a constitutional provision.

Prior Research

From both a policy perspective, as evidenced by the proposed reforms in Arkansas and Utah, and in the scholarly literature, deliberation about the proper selection mechanism for judges continues. The bulk of modern scholarship in opposition to judicial elections continues to come from the legal community (see, for example, Carlton 2003; Fitzpatrick 2018; Friedman 2009; Geyh 2003, 2019; Kowal 2016; Pozen 2008; Sample et al. 2007; Shugerman 2012).

Legal scholars' normative arguments against the practice of electing judges are rooted in idealized longings for absolute judicial independence rather than based on empirical, scientific evidence (see Hall 2001 for a similar criticism). Indeed, much research from political science and other empirical disciplines rejects many of reformers' various claims. Reformers argue that elite appointments and non-partisan elections grant judges independence—i.e., freedom from political pressures and the whims of voters. However, research shows that non-partisan elections do not strip politicization from the judicial selection process (Canes-Wrone et al. 2014; Hall 2001) and that elite appointments do not free judges from political pressure (Blake 2018; Gray 2017). Reformers contend that appointment produces judges of better temperament and more merit, but research shows that that no substantive differences exist in the qualifications of appointed versus elected judges (Canon 1972; Glick and Emmert 1987; Goelzhauser 2016).

Reformers further argue that judicial elections do not even provide an accountability mechanism for the public, as advocates claim (Baum 1983; Flemming 1998; Gambitta et al. 1981; Hall 1984; Mather 1998; Oakley 2009). However, scholarship shows that elections do provide citizens with meaningful accountability (Bonneau 2007; Hall

2001; Hall and Bonneau 2006). For example, Canes-Wrone et al. (2014) study the effect of varying judicial selection institutions on the upholding of death penalty cases at the appellate level. In their investigation of state courts of last resort, they find that judges selected via non-partisan elections upheld the capital sentence more frequently than judges selected under other institutional arrangements, echoing previous findings from Brace and Hall (1995) (see also Brace and Boyea 2008). Park (2017) investigates racial disparity in sentencing in Kansas. Park's results indicate that judges selected by the electorate tend to be more punitive toward Black defendants in time periods closer to an election than are judges in the 17 appointed districts. These results provide strong evidence that judges take into consideration electoral concerns, a hallmark of accountability, especially on salient issues (see also Gordon and Huber 2007; Huber and Gordon 2004).

Research on the consequences of judicial selection mechanisms has been developed in small pieces over the years. Early scholars explored whether elections produced judges who differed in terms of quality, experience, or diversity than appointed judges and found that much of the variation that existed was more likely a result of geography than selection institutions (Canon 1972; Glick and Emmert 1987). Recent research has served to confirm that little to no differences exist between the characteristics of appointed judges and elected ones (Goelzhauser 2016). Other research has investigated the behavior of judges on the bench and found that selection systems are unassociated with opinion clarity, even in high salience cases (Goelzhauser and Cann 2014); that selection mechanism plays no role in predicting whether judges in other states will cite a judge's opinion (Hinkle and Nelson 2016), indicating that judges themselves may not see a meaningful difference in quality based on the selection mechanism of their peers; and that elections are not associated with

differences in dissent (Hall 1984). Research has also investigated the political (Bonneau and Hall 2009; Hall 1984; Hall 2001; Rock and Baum 2010) and mass attitudinal (Gibson 2009, 2012; Nownes and Glennon 2016) consequences of selection mechanisms.

Much of the research on state courts has focused on the policy implications, and this research continues that tradition. Studies find that judicial elections are associated with greater punitive policies in the states, including higher sentencing rates (e.g., Gordon and Huber 2007; Huber and Gordon 2004), larger racial disparities in sentencing (e.g., Park 2017), and greater use of capital punishment (e.g., Brace and Boyea 2008; Brace and Hall 1995; Canes-Wrone et al. 2014) in addition to responsiveness to public opinion in cases dealing with other politically salient policy issues (e.g., Caldarone et al. 2009; Canes-Wrone et al. 2012).

Prior research on judicial policymaking in the states has been limited, however, in at least two ways. First, the research on policy outcomes has focused on appellate courts, especially on state supreme courts and the justices who serve on them (Brace and Boyea 2008; Brace and Hall 1995; Caldarone et al. 2009; Canes-Wrone et al. 2014; Canes-Wrone et al. 2012; Canes-Wrone et al. 2018). Such a research program is understandable. Data are more easily obtainable and more readily available for only 52 state courts of last resort, and on the 338 justices who sit on them, than on the hundreds of trial courts and tens of thousands of trial judges across the country.[8] While this research has laid a firm scholarly foundation, it does not tell us anything about trial courts, where millions of cases are heard every year.

[8] Forty-eight states have a single court of last resort, or state supreme court. Two states, Oklahoma and Texas, have two courts of last resort—one for civil matters and one that hears criminal cases.

Second, the research that has been conducted on trial court judges has been limited to studying a single state (Gordon and Huber 2007; Huber and Gordon 2004; Park 2017). Data availability certainly makes a single-state study more tractable than a large-scale comparative research design. Moreover, one state, Kansas, is a methodologically advantageous state ripe for studying the effects of judicial selection. Trial courts in Kansas are arranged in 31 judicial districts. Judges in 14 of the districts are selected via partisan elections while in the other 17 judicial districts, judges are appointed. However, these one-state studies do not permit generalizing the findings to other states. Put more succinctly, previous research has taught us much about the effects of various judicial selection and retention schemes, but we still know very little about how these mechanisms play in trial courts, and what we do know about trial courts comes from just a couple of states. The current project attempts to fill these gaps in our knowledge about the policy implications of trial court elections.

Strategic Theory: Linking Public Opinion and Judicial Elections

The choice to investigate policy outcomes necessitates the identification of a relevant theoretical lens through which to conduct such an investigation. Because policy outcomes are frequently responsive to public opinion, the strategic model of judicial behavior (Epstein and Knight 1998) provides the sort of framework required. According to this theory, judges ultimately arrive at a final outcome—a vote, an opinion, a ruling—only after a series of decisions about how to account for the preferences of other actors in the political and legal systems. Epstein and Knight (1998) write that judges "are strategic actors who realize that their ability to achieve their goals depends on a consideration of the preferences of other actors, the choices they expect others to make, and the institutional

context in which they act" (10). In my account, "the preferences of other actors" is the public's view of crime and punishment. "The choices they expect others to make" is the threat of removal from office. And "the institutional context in which they act" is the presence or absence of elections to maintain their seat on the bench. Indeed, Epstein and Knight recognize the importance of public opinion on the behavior of jurists: "Because they operate within the greater social and political context of society as a whole, [judges] must attend to those informal rules that reflect dominant societal beliefs about the rule of law in general and the role of [courts] in particular" (157).

Of course, Epstein and Knight's account was of the Supreme Court and the justices who staff it. But the logic of their theory applies equally—if not better—to lower court judges. When the people play a direct role in the selection and retention of judges, they become a critical "other actor" in the system, and judges should be inclined to consider their preferences. Examining how considerate judges are of public opinion can reveal important insights into the efficacy of institutional design.

Democratic theory contends that government of the people should reflect the will of the people. Judges who are subject to democratic selection and retention, thus, should, according to democratic theory, be responsive to public opinion. The strategic theory of judicial behavior offers the same prediction. In the realm of criminal justice policy, I expect to find that in states where judges are retained by popular election, whether it be through an uncontested retention election, a non-partisan election, or a partisan election, criminal justice policy will be more responsive to public opinion than in states where judges do not face the public to retain their seat on the bench.

A final note on these expectations that elected jurists will be responsive to public opinion is that my intent is to show the impact of institutional design on a small segment of the criminal justice system, rather than to judge the prudence of judicial elections. Many scholars have weighed in on the wisdom of electing judges. Bonneau and Hall (2009) dedicate an entire book-length treatment to defending the practice of judicial elections, while Geyh (2003, 2019) has on multiple occasions sought to rhetorically dismantle the system brick by brick. Some scholars have even stumbled into their position while on an endeavor to take the opposite side of the argument (Gibson 2012; Shugerman 2012). I will leave to others the position of evaluating the merits of the system as the sense of taste—or the sense of smell (Geyh 2003)—will vary from person to person. And the empirical results I present in the chapters that follow provide fodder for both proponents and opponents of judicial elections.

Analytical Considerations and Decisions

In the research contained in this work, several analytical considerations were taken into account, and I had to make decisions that facilitated the ability to answer questions about the role of public opinion and judicial elections in shaping state criminal justice policy. The first decision was to explore policy implications rather than analyzing some other outcome of political or social importance, such as vote shares or judicial diversity. Second, I chose to investigate criminal justice policy in the states rather than at a national or cross-national level. Third, I analyze the role of trial court elections rather than the implications of elections for supervising courts. Fourth, rather than studying the impact of elections in the initial selection of trial judges, I focus on the ramifications of retention elections. The sections that follow expand on these decisions.

Why Study Policy Outcomes?

At least two paths exist to explore the wisdom of judicial elections. The first path is to investigate policy outcomes. Such an analysis has a well-established tradition in the political science literature and beyond (e.g., Brace and Hall 1995; Brace and Boyea 2008; Caldarone et al. 2009; Canes-Wrone et al. 2012; Canes-Wrone et al. 2014; Gordon and Huber 2007; Huber and Gordon 2004; Park 2017). Other scholars have taken alternative approaches, investigating other important matters, such as voter behavior (Rock and Baum 2010), electoral outcomes (Hall 2001; Hall and Bonneau 2006), judicial characteristics (Canon 1972; Glick and Emmert 1987; Goelzhauser 2011, 2016), the clarity of written judicial opinions (Goelzhauser and Cann 2014), and the legitimacy afforded to the institution (Gibson 2009, 2012).

Although policy outcomes are by no means the only outcomes of interest in judicial elections research, I explore policy outcomes because of their burning normative implications and the empirical questions they raise. Why is incarceration so high in the United States? Why do racial minorities experience more harsh punishment than members of the racial majority? What, if any, reforms can we make to the system to alleviate these issues? The ability to shine some light on these issues motivates my decision to study policy outcomes.

Studying the States

Questions about how judicial elections and public opinion shape criminal justice policy are difficult to answer in a large cross-national comparative analysis for at least two reasons. First, and most importantly, little variation exists in judicial elections across countries. While the United States is not the only nation in the world that elects judges, it

is the only one that does so on such a wide-scale basis. Second, legal systems vary greatly from nation to nation, so a lot of the variation in policy outcomes can be attributed to legal differences too numerous to account for. Fortunately, the variation across the American states provides a more manageable research design. Since the Civil War, reforms have been made at infrequent intervals to state judicial selections mechanisms, efforts that ramped up quite dramatically during the Progressive Era and through the middle years of the 20th century (Geyh 2019; Goelzhauser 2016; Shugerman 2012). Alterations in judicial selection and retention have died down since the 1980s, although six states have made reforms in the intervening years (National Center for State Courts 2018).

In many ways, however, state judiciaries—and states themselves—are very much alike. And in the ways that states, their legal climates, and their judiciaries do differ, comparable data from similar or identical data sources are available. All state judiciaries are situated in a separation of powers environment in which they share governing powers with state executives and state legislatures. The focal point of this project is state trial courts, and all trial judges serve in district-based, local courts, and are subject to the threat of review and reversal by at least one supervising, i.e., appellate, court.

States themselves and their legal systems are also alike in more ways than they are different. While the "live and let live" states of the Mountain West may seem strikingly different from the "lock 'em up and throw away the key" legal systems of Texas and Louisiana, the states operate under similar legal constraints, e.g., the US Constitution and federal statutes, and the society of each state is relatively comparable socioeconomically and politically to every other state. The social and political similarities allow me to study the key characteristics that do vary from state to state and isolate the effects of the

characteristics of interest, namely the retention mechanism for trial judges and the level of punitive mood among the public and answer the questions about judicial responsiveness to public opinion and whether that representation is more prevalent in institutional arrangements that promote democratic accountability.

Why Study Trial Courts?

Trial courts are the heart of the American judiciary. In much of the existing literature on judicial elections, however, scholars have studied appellate, rather than trial, courts. This is especially true of scholars who investigate policy-related outcomes, such as abortion law (Canes-Wrone et al. 2012), or the upholding of capital sentences in criminal cases (Brace and Hall 1995; Canes-Wrone et al. 2014). The current study investigates the role of trial courts, rather than appellate courts, for several important reasons. First, trial courts are the venue in which sentencing decisions are made. Trial judges sentence convicted defendants, often handing down time in the state's prison system, which has become the largest per capita prison system in the world. Thus, trial courts are the appropriate level of analysis for studying incarceration rates and the resulting racial disparities as outcomes.

Second, state trial courts are the workhorses of the American judiciary. They handled 99.2% of the caseload in American courts in 2015, a caseload of 86.2 million cases, while state appellate courts heard a fraction of that number, with a caseload of 260,000 (Schauffler et al. 2016). That same year, the entire federal courts system handled a total caseload of only 418,379 ("Federal Judicial Caseload Statistics" 2015).

Third, state trial judges often seek and win seats on their states' appellate courts. Trial court experience signals to voters the credibility and quality of a state supreme court

candidate, even holding other factors constant (Hall and Bonneau 2006). Voters, in response to this signal of credibility, reward supreme court candidates with trial-judge experience with higher average vote shares. Because trial judges interested in moving up the ranks of the state judiciary are likely to be successful in doing so, studying trial courts is especially important.

Fourth, the mechanism for judicial selection and retention can vary within state by the level of court. For example, in California, justices of the supreme court are initially seated via gubernatorial appointment subject to confirmation by a judicial commission composed of the chief justice, the state attorney general, and the chief judge of the appellate court, while trial judges are selected via non-partisan elections. While all judges are retained via election, the trial judges are re-elected in contested non-partisan elections, and the supreme court justices are retained via uncontested retention elections. In an even more complicated scheme, New York's supreme court judges are selected via elite appointment, while trial judges are selected in partisan elections. The Empire State then retains its justices and judges via the same manner by which they were initially selected. Thus, all judges in California will face voters at some point, while in New York, some judges will face voters at regular intervals while other judges will never be on a ballot. Given this within-state variation, the results of research conducted on supreme courts will not necessarily generalize to the trial courts, even in the same state.

Selection versus Retention

Scholars must make a distinction between selection and retention institutions for state judges depending on the outcome being studied (Goelzhauser 2016, 2018). For example, initial selection is more likely to influence important factors such as the

qualifications of judges seated or the racial composition of the bench (Canon 1972; Glick and Emmert 1987; Goelzhauser 2016). The retention phase, or the prospect of having to face voters in the future, might affect how judges behave on the bench, such as how clearly they write their opinions if they believe those opinions might be used by opponents in future campaigns (Goelzhauser and Cann 2014). Where the institutions differ, as they do in many states, capturing the effect of the phase—either selection or retention—most likely to influence the outcome of interest is critical. Since incarceration rates and racial disparity in incarceration rates are influenced by judges on the bench, judges likely punish based on prospective electoral concerns, as informed by the public's punitive mood, rather than retrospective considerations of how they came to sit on the bench. Therefore, I focus on retention elections in this project.

Overview of the Dissertation

The role of retention mechanisms for trial judges and public opinion for determining criminal justice policy is a constant theme in this research. This work's three empirical chapters probe the relationship between at least two of these factors. The exploration of these relationships is as follows:

In chapter 2, before undertaking the statistical analyses I develop a democratic theory of judicial behavior, and how, at the state level, judicial institutions are likely to shape criminal justice policy outcomes. Following the theoretical foundation of Epstein and Knight (1998), I theorize that elected judges concern themselves with re-election and thus behave strategically on the bench to appeal to the electorate. I further theorize that state electorates vary in their level of preferred punitiveness, an important factor that judges must take into consideration, depending on their retention mechanism. From the

democratic theory of judicial behavior, I posit a series of testable hypotheses predicting that states with elected judges will be more responsive to public opinion than states without judicial retention elections.

I theorize in chapter 2 that public opinion is dynamic—that it is different from state-to-state and that opinion changes over time. Having a measure of public opinion about crime and punishment that varies across states and over time is critical to my research. chapter 3 therefore introduces the Punitive Attitudes Dataset for the States (PADS). This original dataset measures punitive mood that varies both across states and years. In other words, the measures of punitiveness vary both spatially and temporally, an improvement over previous measures of punitiveness that differ on only one of these dimensions.

In chapter 4, I test hypotheses relating to state incarceration rates. Using time-series regression analysis, I show that, compared to states without elections, states where judges are retained via election produce greater responsiveness to public opinion. As states become more punitive, incarceration rates rise. I then test the hypothesis that when a state changes its judicial retention method, incarceration rates should be causally altered. I conduct difference-in-differences analyses for three states that changed their retention mechanism from partisan to non-partisan elections, but find no evidence of a causal relationship.

Chapter 5 follows a similar analytical path, testing hypotheses predicting that racial disparities in incarceration are also subject to change based on public opinion. My time-series regression analyses demonstrate little support for the hypotheses that judicial elections are associated with greater racial incarceration disparities. Compared to states without elections, states with elections are no different in their levels of Black-to-White

disparity. However, non-partisan elections are associated with higher levels of disparity than partisan elections, conditional on public opinion. My difference-in-difference analysis of electoral reform demonstrates that in one state, however, changing from partisan to non-partisan elections causally decreased racial disparities in the state prison system, and I explore a potential explanation for this finding.

I close this work by reviewing and synthesizing my findings to discuss their important implications for institutional design, criminal justice policy and policy-making. After summarizing my findings, I connect them to the argument made by both proponents and opponents of judicial elections. I conclude by discussing a path for future research on this topic, especially research that incorporates the novel PADS data introduced in chapter 3.

Chapter 2: Theory, Data & Methods

Introduction

As explained in chapter 1, fueling the debate between judicial accountability and judicial independence is the question, to whom, if anyone, should judges be accountable? For the most part, this question is answered by a polity's choice of which judicial selection and retention mechanisms to adopt. Jurisdictions that favor absolute judicial independence should adopt a scheme that provides for a seemingly apolitical appointment process followed by institutional protections such as lifetime tenure and protected salaries. On the other hand, jurisdictions that favor judicial accountability should adopt some form of elections. Democratic theory predicts that elected judges will be more responsive to public opinion than jurists who do not face election, and previous research shows support for this contention. However, much of that research explores the responsiveness of state courts of last resort. In chapter 1, I presented a case for studying state trial court elections.

In this chapter, I develop a theory of judicial democratic responsiveness. Trial judges have a distinct role in the American legal system compared to appellate judges. One distinction critical to the theory I develop in this chapter is their relative autonomy. Appellate judges and justices work on collegial courts and, as a result, their colleagues' expected behavior factors into their strategic calculus. In other words, they have to concern themselves with how other judges on the case may want to decide the case. Thus, appellate judges have more to consider when making a decision than do trial judges. By contrast, trial judges are autocratic to the extent that they do not have to be concerned with having multiple judges on the same case. While they do have to worry to an extent about their decisions being reviewed by appellate courts and potentially reversed, a prospect I consider later, trial judges are relatively free from the influence of other judges, thrusting the

importance of public to the fore. Thus, while much of the theory developed here certainly could apply to appellate judges, it is most applicable to trial judges because of their somewhat unique position in having to consider public opinion.

Of course, studying trial courts at the local level presents several important data problems. First, data on sentencing decisions, including type of sentence and sentence length, are not publicly available on individual judges or by judicial district. Thus, obtaining a reliable estimate of some outcome of interest is nearly impossible. Second, and more critical, is the ability to measure public opinion at the level of the judicial district in every state. While, as I will show in chapter 3, advances in estimating public opinion have made measuring public opinion at the state level more plausible, the techniques used to do so may not be reliable at the sub-state level due to having so few respondents in many states. Indeed, to my knowledge, only one study has measured public opinion at a more local level, and those researchers did so only in cities with fairly large populations that are overrepresented in public opinion surveys (Tausanovitch and Warshaw 2014). So, while estimating public opinion at the state level surely obscures important district-level variation that should influence trial judges, studying state-level opinion is a reasonable alternative strategy.

In the development of my theory, I derive several testable hypotheses that predict greater responsiveness to public opinion of states that elect trial judges. I hypothesize first that, since in most states and most years, Americans tend to prefer punitive responses to crime and elected judges perceive this preference, states with elected judges should, on average, have higher levels of punitive policy outcomes than states where judges are not elected. I then hypothesize that the impact of judicial retention systems on punitive

outcomes (e.g., incarceration rates) is also conditional to some degree on the actual level of public punitiveness within a state, such that states that elect judges should exhibit greater punitive policy outcomes when the public mood skews more toward punitiveness.

Following the theory section, I discuss the data used to test these hypotheses, which is drawn from two primary sources. The first is the National Prisoners Statistics (NPS) dataset collected by the US Bureau of Justice Statistics, which is a state-year (time-series, cross-sectional) census of prison populations. I use these data to construct several of the primary dependent variables, such as *Incarceration Rate* and *Black Disparity*, which are used in the analysis. The second source of data is the Punitive Attitudes Dataset for the States, an original dataset of public opinion first introduced in this project. Based on dozens of national surveys that assess the public's punitive attitudes toward crime, I use multi-level regression and post-stratification (Gelman and Little 1997) to estimate measures of the public's punitive mood over time in the American states. This is the first dataset that measures punitive mood dynamically over time and space.

After discussing the measurement of dependent, independent and control variables used in my analyses, I detail the analytical strategy for testing the hypotheses. Specifically, I use linear regression with panel-corrected standard errors (Beck and Katz 1995) to examine time-series evidence for my hypotheses. I also use difference-in-differences (Card and Krueger 1993) analysis with synthetic control groups (Abadie et al. 2010) to investigate how state prison populations change following a reform in judicial retention mechanisms.

Theory

Crime is frequently a salient issue in American politics (Costelloe et al. 2009). Indeed, when the economy is doing well, crime is frequently cited in public opinion polls as the most important issue facing the nation (Levitt 1997). Although the number of Americans who view crime as the *most* important issue facing the country has dropped in recent years according to Gallup trends (Jones and Saad 2018), most people still say crime is a somewhat or very important issue. Take, for example, the Views of the Electorate Research Survey (VOTERS) conducted by the Democracy Fund Voter Study Group in 2016, when the economy had vastly improved from its low point in 2007. In that survey, 92.5% of respondents listed crime as either a "very important" or "somewhat important" issue to them (Democracy Fund Voter Study Group 2017).

Not only is crime a salient issue, but it has an important political impact on politicians' reelection fortunes. Cummins (2009) has demonstrated, for example, that state crime rates are an important predictor of the outcomes of gubernatorial elections, arguing that state levels of crime are more salient to state electorates than the national crime rate. The political importance of crime is also evidenced by the findings of Petrocik (1996) that, from 1960 through 1992, politicians of both political parties devoted nearly a quarter of their campaign messaging to matters of crime and justice, a finding confirmed by Petrocik et al. (2003) using expanded time series from 1952 through 2000. So it seems that candidates and officeholders understand the salience of crime to their constituencies.[1] As

[1] While Republicans initially "owned" the issue of crime and punishment, after the sound defeat of Michael Dukakis in 1988, most national Democrats moved to the right on the issue. During the Clinton years, Democrats reduced the substantial advantage Republicans had on the issue of crime (Sides 2006), but they did so primarily at the symbolic level and less so at the policy level. According to Holian (2004), Democrats closed in on Republican

Peffley and Hurwitz (2010) put it, elected officials generally are "unusually cognizant of, and responsive to citizens' attitudes toward crime" (11).

The Study of Punitive Attitudes

The salience of crime has persisted for several decades and is frequently cited by public opinion poll respondents as among the top issues facing the nation and their community (Democracy Fund Voter Study Group 2017). This is especially true when the economy is doing well (Levitt 1997). Moreover, citizens are apparently adept at using crime rates as an indicator of incumbent performance in their vote choice. Specifically, Cummins (2009) reports that voters are highly responsive to state crime rates and electorally punish incumbent governors when crime rates are high.

The importance of crime among the citizenry is not lost on politicians clamoring for votes. Politicians are "responsive to citizens' attitudes toward crime" (Peffley and Hurwitz 2010, 11), and dedicate a substantial portion of their advertising to the issue (Petrocik 1996; Petrocik et al. 2003). Additionally, criminal justice policies in the states are among the most responsive and congruent to public attitudes. Lax and Phillips (2012) show that the death penalty—one of the measures of public opinion I use—is both highly responsive to public opinion (i.e., the death penalty is in practice in states where citizens favor its use) and is one of the more congruent policies (a substantial bias in public opinion is not necessary to bring about the policy) at 72%. Moreover, Enns (2014, 2016) has

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credibility by (1) accepting the importance of capital punishment, thus "gaining credibility to enter the public debate regarding crime" (101), and (2) arguing that, in addition to increasing punishment and cops on the street, there was more to handling crime than just punishment. Indeed, Holian's finding is that even during a period when Democrats were more credible on the issue of crime, they accomplished this more by a change in rhetoric than a dramatic change in policies. Again, they largely dropped their opposition to the death penalty and emphasized prevention over punishment in a "yes, but" strategy (101).

demonstrated that criminal justice outcomes, i.e., incarceration rates, are highly responsive to public punitive opinions. When public opinion becomes more punitive, incarceration rates rise; when the public expresses less punitive attitudes, incarceration rates tend to fall (see also Jennings et al. 2017; Johnson 2001).

Considering the importance of crime in American politics and elite responsiveness to the public's preferences, capturing the public's punitive attitudes is critical to understanding criminal justice policy and punishment in the United States (Drakulich and Kirk 2016; Frost et al. 2019; Gromet and Darley 2011; Wozniak 2015).

While a number of considerations influence Americans' support for crime policies, a central consideration is the degree to which the policy focuses on the punishment or rehabilitation of those who commit crimes. On one end of the spectrum, people prefer the justice system to provide incarcerated citizens with rehabilitative services, such as education and workforce training, to permit successful reentry into society and reduce recidivism. On the opposite end of the spectrum, citizens are inclined toward a more punitive approach to crime, supporting longer prison sentences and the use of capital punishment as a means to either deter crime or provide just desserts for offenders.

Although Americans' attitudes about crime and punishment vary from person to person, opinions in the aggregate are skewed toward the punitive end of this dimension (Enns 2014, 2016; Johnson 2001; Schaefer and Uggen 2016), particularly when media coverage focuses disproportionately on violent crime stories (Tonry 2004). Moreover, attitudes toward criminal punishment vary both from time-to-time and place-to-place. Public opinion—about crime, abortion, government spending, or any other policy area—

varies both spatially and temporally. In other words, punitive attitudes are not the same in every place at all times.

Public mood on crime and punishment first varies across states. The "live and let live" attitudes of the upper Mountain West states are different than the "lock 'em up and throw away the key" mantra of Texas. Thus, understanding attitudes toward crime and punishment is especially critical in the states, where most criminal justice policy is set and the majority of criminal punishment is implemented. Furthermore, understanding punitive opinions in the states permits a comparative examination of elite responsiveness to those attitudes. Put another way, state-level measures of punitive mood provide variation that is essential to the scientific study of crime, justice, and punishment and the democratic responsiveness (or lack thereof) of elites.

"Tough on crime" attitudes also vary over time. Punitiveness in 1965 when President Lyndon Johnson declared a "war on crime" was not the same as punitiveness in 1982 when President Ronald Reagan declared a "war on drugs," and in 2017 when President Barack Obama left office following the first major decline in federal prison populations in decades, punitiveness was different still.

To understand the impact of public opinion vis-à-vis punitive responses to crime on the outcomes observed in the criminal justice system and the role political elites play in shaping those outcomes, we must have data that vary on both the time and space dimensions. However, many studies of punitiveness and policy responsiveness to punitive mass attitudes have not exploited this variation. Data that vary spatially are often treated as temporally static (see, e.g., Lax and Phillips 2012), while data that are temporally dynamic are measured in the aggregate and not permitted to vary spatially (e.g., Ramirez

2013). Enns (2014), for example, demonstrates the responsiveness of governments to punitive attitudes by reporting a rise and fall in incarceration rates concordant with rising and falling levels of mass punitive opinion. However, both his measures of punitiveness and incarceration are pooled across states, forgoing one of the important aforementioned source of variation. Ramirez (2013) estimates "punitive sentiment" across time to explore the antecedents of providing a punitive response in a public opinion poll. However, he similarly ignores any cross-state variation in levels of punitive opinion and provides only pooled, national estimates.

Some studies, do, however, exploit cross-state variation in public opinion to estimate public attitudes toward crime and punishment. Lax and Phillips (2012) conduct multi-level regression and post-stratification (MRP) analyses (Gelman and Little 1997) on 39 policy areas, including a several criminal justice issues (see also Enns and Koch 2013; Kastellec et al. 2015; Lax and Phillips 2009; Pacheco 2011; Tausanovitch and Warshaw 2014). These analysts exploit this state-level variation in opinion to explore policy responsiveness and congruence to the various policy issues. However, their analysis fails to account for another, equally important, source of variation in punitive attitudes, one that was explored by scholars such as Enns (2014, 2016) and Ramirez (2013): time.

Just as cross-state variation is important for understanding punitive opinions, so also is temporal variation. Over-time variation is important for at least two reasons. First, allowing the punitive attitudes of the public to vary over time permits researchers to evaluate the temporal responsiveness of elites to public opinion. Enns (2014) demonstrates that tough-on-crime attitudes at *t-1* indicate a strong and significant positive relationship

with incarceration rates at time *t*, controlling for other political and social factors. A temporally dynamic measure of punitive opinion permits such an analysis.

Second, punitive attitudes simply are not fixed. Even within a state, attitudes toward tough-on-crime policy can fluctuate, as does elite rhetoric on the topic. For example, at the national level, elites have shifted positions on crime. During the Reagan and George H.W. Bush administrations, Republicans were tough on crime and Democrats were considered "soft" on crime, preferring prevention and rehabilitation to punishment (Petrocik 1996). During the Clinton administration, Democrats erased the GOP advantage on crime by accepting punitive policies as an essential response while arguing that there was more to dealing with crime than harshly punishing criminals (Holian 2004). In the post-Clinton era, Democrats moved back to the left on the issue accompanied by a liberal shift by Republicans as well (Holian 2004). The Republican shift was driven by major players in right-of-center politics, such as Charles and David Koch, who spun criminal justice reform in cost-savings terms (Nwanevu 2018) and accelerated as the Republican Party took advantage of new wedge issues, especially terrorism and (later) immigration (Dagan and Teles 2013). For these reasons, temporally dynamic measures of punitive opinion are important.

Retention & Incarceration

In the aggregate, mass support for punitive policies in the US has been a major driver of the country's distinction of having the highest percentage of its population behind bars (Lappi-Seppälä 2018, 239). In fact, research shows that incarceration rates in the US tend to be responsive to the percentage of the public that supports punitive policies—such as support for the death penalty or opposition to legalizing marijuana—to combat crime

(Enns 2014, 2016; Jennings et al. 2017). Thus, as public opinion becomes more (less) punitive, the incarceration rate at the federal and state levels increases (decreases) over time, but incarceration rates fall more slowly due to longer sentences from earlier, more punitive periods. To be sure, many state and federal actors and institutions are responsible for policymaking in the criminal justice arena, but only trial courts and the judges who staff them are directly responsible for state incarceration rates since they play a more direct role in sentencing defendants. Thus, if public opinion plays a role in driving rising or falling incarceration rates, it is likely to be through elected trial court judges as well as electing gubernatorial and legislative office-holders.

Following the advice of Goelzhauser (2018), I distinguish between selection and retention mechanisms in the states, which often differ. Because my theoretical focus is on judicial retention, technically all three types of elections are "retention elections." To properly distinguish the nominal retention election in the remainder of this paper, I employ the term "uncontested retention election."

Scholars have long argued that the foremost goal of elected officials is to win re-election (Mayhew 1974). A reasonable assumption is that this objective also applies to elected judges who seek re-election, as they must do in 38 states. Thus, judges seeking re-election, their opponents, and interest groups supporting or opposing one of the candidates must inform the electorate about the incumbent's behavior in office through campaign communication, and the ability of judges to campaign on issues that might come before them on the bench was sanctioned by the US Supreme Court in its 2002 *White* decision.[2]

[2] *Republican Party of Minnesota v. White* 536 US 765 (2002)

One such advertisement, a catchy ditty set to banjo music, run by an independent expenditure group for a North Carolina judicial race claimed that Judge Paul Newby, the group's supported candidate, "put criminals on the run," warning them, "criminals best beware" of the judge, who doled out "justice, tough but fair."[3] Of course, supportive ads, no matter how clever or catchy, do not have quite the emotional appeal of a hard-hitting negative ad (Geer 2006). On a negative tone, the Republican State Leadership Committee entered the Wisconsin Supreme Court race in 2020, attacking candidate Jill Karovsky with an ominous television ad as "dangerously soft on crime" for going "easy on criminal predators" during her time as a prosecutor.[4]

Clearly, campaign advertisements are making an appearance in judicial races, at least in the highly salient races for state supreme courts. Moreover, those ads are being used to raise criminal justice themes in an effort to sway voters (Caufield 2007). Evidence even suggests that campaign advertising on criminal justice matters in judicial elections can have an effect on voters' choice of candidate (Reid 1999). However, given the lower salience of trial court contests (Nelson 2011) and the lower levels of spending (Bonneau and Cann 2015), the question becomes whether electioneering for trial court judgeships features similar patterns of communication. Despite earlier studies suggesting that they do not (Abbe and Herrnson 2002; Arbour and McKenzie 2010, 2011), one recent study shows that over 25% of trial judges used "crime policy and protecting the community" language in their campaign communication (McKenzie and Rebe 2017, 165). Future research is certainly warranted to answer this question, especially on the extent to which voters are

[3] https://www.youtube.com/watch?v=Zkoi0Vg7caw
[4] https://bit.ly/2L7ju3K

made aware of the sentencing behavior of individual trial judges by campaign communications. However, despite the mechanism by which trial judges inform the voters, the desire for re-election and the requisite strategic behavior on the bench to attain re-election are safe assumptions.

These assumptions are supported by empirical studies. For example, Huber and Gordon (2004) investigate more than 22,000 criminal cases in Pennsylvania during the 1990s and find that "the sentence imposed by a judge whose election is imminent is likely to be about three to four-and-one-fourth months longer…than if the judge were recently elected or retained" (255). In sum, they attribute approximately 2,000 "years of incarceration to the electoral dynamic" (Huber and Gordon 2004, 247). In another study, Gordon and Huber (2007) show that, from 1997 to 2003, Kansas judges in electoral districts exhibited a probability of assigning a prison term up to 4.01% higher than judges in uncontested retention districts, and judges who faced an electoral challenger handed down prison sentences that were up to 3.7 months longer than uncontested judges. Moreover, Park (2017) demonstrates that judges in Kansas who must compete for re-election sentence Black defendants to incarceration 2.4% more frequently than whites in the final six months of an election cycle. Similar results were uncovered by Brace and Hall (1995) and Canes-Wrone et al. (2014), who demonstrated that elected state supreme court justices uphold capital sentencing decisions more frequently than justices who are appointed, and elected judges respond to majority opinions on the death penalty in their states (see also, Brace and Boyea 2008; Kritzer 2016; Nelson 2014).

Thus, judicial candidates who must convince the public to vote for them must also demonstrate to the public that they are listening, and responding, to the preferences of the

voters.[5] And, while not all states are composed of punitive electorates, a majority are. For example, data from the PADS show a range of punitiveness in the states from .44 to .67, so even in states where a majority of the population is not punitive, strong pluralities are. Therefore, punitiveness in policy outcomes is likely to be higher in elected states independent of actual public opinion. Thus, I propose H1, the *Elections Incarceration Hypothesis:*

> Incarceration rates will be higher in states with judicial elections to retain trial court judges than in states without judicial re-election.

However, not all elections are alike. Research shows that it is important to distinguish among the three types of judicial elections in the states: retention, non-partisan and partisan. *Uncontested retention elections* follow the appointment of a judge by a governor or legislature. In this arrangement, an incumbent judge, after serving a period of time on the bench following elite appointment, faces the voters. The voters are proffered a yes-no question about whether or not the judge should be retained, and the judge does not face a challenger on the ballot. In states with *non-partisan elections*, judicial candidates are proscribed from identifying their party affiliation in campaign communications and the party affiliation is not listed on the ballot. Conversely, in *partisan elections*, judicial candidates are permitted to run under a party label and invariably do so in practice. While my analysis differentiates between uncontested retention, non-partisan and partisan elections, the distinction between partisan and non-partisan elections has long been one of

[5] Some states reduce the discretion afforded to judges in making sentencing decisions through mechanisms such as mandatory minimum statutes or sentencing guidelines. The impact of these legal mechanisms is explored in the appendix (c.f., tables A4-4 and A5-5) to permit a focus on the effects of institutional arrangements in the main text.

the most contentious aspects of judicial elections and a focal point of normative debate (Shugerman 2012).

Differences between partisan and non-partisan elections not only enjoin normative issues, however, but theoretical and empirical issues as well. The theoretical foundation begins with the primacy of party identification in American politics (Bartels 2000). Voters at the individual level are notoriously uninformed about politics (Campbell et al. 1960; Delli Carpini and Keeter 1996; Lewis-Beck et al. 2008). Rather than seeking detailed information about candidates and issues in a fairly large number of elections, comparatively speaking, US voters rely heavily on the heuristic of their party identification to select candidates and evaluate issues and performance information (Bartels 2000). Partisan cues are especially important in general election contests in the US. Like most heuristics, people need at least a modest amount of information to use them in the first place. And like other short-cuts, they can lead to systematic errors in perception and decision-making (Dancey and Sheagley 2013). But at least when partisan cues are available, such as the general election of presidents, voters are able to "vote correctly" about 75% of the time (Lau and Redlawsk 1997; Lau et al. 2008). That is, they vote consistent with their own policy preferences. When partisan cues are not available, however, correct voting declines significantly. Of course, this correct voting occurs in an electoral environment marked by both high visibility and, more importantly, the availability of party heuristics.

By definition, non-partisan elections are void of that critical heuristic for voters. How, then, do uninformed voters make their decision at the ballot box when deprived of a valuable clue as to the candidates' positions on the issues? Imagine in a counterfactual

world a non-partisan election for president. Surely, because of the high-profile nature of these contests, voters would be able to differentiate between the candidates and still vote "correctly" much, if not most, of the time. However, judicial elections are low-information contests that lack the visibility of high-profile campaigns for other up-ballot offices such as president, congress, or governor (Baum 2003; Klein and Baum 2001; Streb 2007; Streb and Frederick 2009). The combination of low-visibility, low-information, and lack of a party heuristic creates an environment where voters face exponentially greater informational costs that they are unlikely to pay for such little benefit (Ferejohn and Fiorina 1974), resulting in the decision to simply not vote at all (Schaffner et al. 2001) or choose not to vote on the judicial races (i.e., roll-off) (Klein and Baum 2001).

How, then, can candidates win an election? The short answer is to campaign. Voters are susceptible to elite appeals (Zaller 1992), including agenda-setting (Abbe et al. 2003), priming (Tesler 2015), and framing (Sides 2006), and that voters often change their own issue positions to match those of their preferred candidates (Lenz 2012). Specifically, campaigns are able to persuade uninformed voters, especially in the last few days of a campaign (Bartels 2008a). And because voters evaluate the past performance of the current administration when making a vote choice—i.e., they vote retrospectively (Fiorina 1981; see also Becher and Donnelly 2013), candidates have an incentive to point to those conditions in their electioneering communications. Indeed, in conditions of non-partisan elections, candidates have been shown to behave in ways that appear highly partisan, even more so than their nominally partisan contemporaries (Crawford 2018). Judicial candidates, thus, are likely to use campaign communications to appeal to voters'

preferences on important issues. And to make their appeals successful, they must perform in office in a way that is consistent with their case for re-election.

Do non-partisan judges respond to public opinion more than partisan judges? Canes-Wrone et al. (2014, 25) claim that "the literature is inconclusive about which judicial selection system creates the greatest plebiscitary pressure." However, judicial scholars repeatedly demonstrate that judges selected via non-partisan elections are often *more* responsive to electoral pressures than partisan judges. For example, Canes-Wrone et al. (2014) report that state supreme court justices elected via non-partisan elections uphold death-penalty cases more frequently than do justices who are selected via other institutions, including partisan elections, independent of a host of control variables. They show that state supreme court justices elected on a non-partisan ballot are more likely to uphold capital sentences in response to state-level majority public opinion on the death penalty.

More generally, research also finds that jurists selected in non-partisan contests are more responsive to public opinion in other policy areas, especially when the policy area is publicly salient (Choi et al. 2010), as is true of issues related to crime.[6] For example, over a nearly 30-year period (1980-2006), state supreme court justices selected via non-partisan ballots were more responsive to state public opinion on abortion than judges selected via other institutional arrangements (Caldarone et al. 2009; see also Canes-Wrone et al. 2012; Crawford 2018). Using public opinion on abortion that was pooled within states over a

[6] See, for example, research by Canes-Wrone et al. (2018) who find that judges, regardless of selection mechanism, are not particularly responsive to public opinion on low-salience issues. Specifically, they examine civil cases in environmental law and demonstrate that judges are only responsive to public opinion on environmental issues when the environment was a topic of attack advertising in the previous campaign, making the issue salient.

decade, Caldarone et al. (2009) report that as the share of the electorate supporting abortion increased across states, so also did pro-abortion judicial rulings; conversely, as the electorate became more pro-life, judges issued more pro-life rulings, even controlling for the judges' party affiliation, election proximity, and case facts. Thus, recent scholarship clearly documents a strong relationship between non-partisan elections and judicial decision-making on salient issues like crime.

Party identification is often the most powerful decision-making heuristic for American voters in general elections (Campbell et al. 1960; Lewis-Beck et al. 2008). Thus, when the party label of the candidates is clear in partisan judicial races, it is possible for many judges to fly under the radar by being somewhat more or less punitive in practice while relying on a partisan cue to communicate to voters that they are in alignment with public opinion on crime. However, when institutional arrangements prohibit the use of that heuristic, as in the case of non-partisan elections, judicial candidates in states with punitive electorates are likely to behave on the bench in a way that is responsive to the perceived attitudes of the public in their state. Judges' actions on the bench provide them with an instant campaign message that gives voters a retrospective accounting of the judge's responsiveness on the bench and a strong case for reelection. Again, this responsiveness is an average response. Thus, I offer H2, the *Non-Partisan Elections Incarceration Hypothesis*:

> Incarceration rates will be higher in states in which judges are re-elected in non-partisan elections than in states with partisan judicial elections.

Retention and Racial Disparities in State Incarceration Rates

Being tough on crime does not inherently lead to a tendency for judges to be harsher on Black defendants than they are on White defendants. For example, a generic anti-crime stance might overcome any tendency to take it easy on White defendants, thereby reducing racial disparities compared to more discretionary contexts. However, numerous studies show that sentencing is often racially disparate. Racial minorities in the United States, especially Black Americans, bear the brunt of the criminal justice system. Blacks in the US are sentenced to longer periods of incarceration than Whites for committing similar crimes under similar circumstances (Mustard 2001; The Sentencing Project 2013), and Blacks make up a disproportionate share of state prison populations (The Sentencing Project 2017; Yates 1997; Yates and Fording 2005).

While crime has long been a salient issue in American elections and public policy, the role of racial resentment in American politics has increased generally in recent years, even in policy areas unrelated to crime. For example, Enders and Scott (2019) report that, since the late 1980s, racial resentment has become increasingly correlated with a number of attitudes (e.g., ideology), policy preferences (e.g., toward general government services), and behavior (e.g., vote choice) among White Americans. They conclude that the "connections between racial resentment and other political variables persist in the face of the relative stability of racial resentment among American Whites" (Enders and Scott 2019, 298).

What is more, the two—crime and race—are inextricably linked in the minds of many Americans, with people associating Blacks with criminality and violence (Duncan 1976; Sagar and Schofield 1980). But does that mean elected judges will necessarily hand

down longer sentences to Blacks? Two streams of literature indicate this may be the case. First, social psychology has accumulated a wealth of evidence demonstrating that most people implicitly associate Blacks with criminality (see, e.g., Allport and Postman 1947; and Eberhardt et al. 2004 for a lengthy discussion of the literature on this association), a bias to which Blacks themselves are not immune (Correll et al. 2002). For example, Eberhardt et al. (2004) show that the association is bidirectional. Priming an experimental subject with exposure to a Black face made the subject more attentive to a weapon, while priming the subject to think about crime made them more attentive to a Black face.

More to the point, Rachlinski et al. (2009) demonstrate that trial judges are not exempt from this implicit bias (see also Rachlinski and Wistrich 2017). These authors administered a series of experimental tests to a sample of judges attending one of several judicial workshops. First, they administered an Implicit Association Test (IAT), asking the judges to first associate White faces with positive words and Black faces with negative words and then to associate White faces with negative words and Black faces with positive words. They then used the response times to calculate an IAT score. The authors report that "white judges expressed a significantly larger white preference" than their Black colleagues (Rachlinski et al. 2009, 1210). They then subjected the judges to a computer based race-priming exercise, in which the judges were rapidly exposed to a series of 60 words that were either associated with Blacks (e.g., jerricurl, Cosby, ghetto) or were race-neutral (e.g., heaven, mosquito, birthday) (Rachlinski et al. 2009, 1213 footnotes 86 and 87).

The researchers also provided the judges in the sample three case vignettes. In the first two cases, the judges were not made explicitly aware of the defendant's race. On these

two vignettes, however, the authors report that judges with more White-preference IAT scores were more likely to hand down a longer sentence if they had been primed with the Black-associated words, evidence the authors take to indicate that implicit bias plays a role in sentencing Black defendants more harshly than White defendants. The third vignette, however, explicitly mentioned the race of the defendant—half of the sample saw a Black defendant, while the other half saw a White defendant. On this case, the authors found no evidence of a relationship between implicit bias and sentencing discrepancy. In short, judges in the sample exhibited an implicit bias against Blacks that many ordinary Americans have. And in some instances, but not all, as demonstrated by the third vignette, that bias can shape their sentencing decisions.

Second, a recent study by Park (2017) found that, between 1998 and 2011, trial judges in Kansas who were running for re-election sentenced Black offenders to incarceration at higher rates than White offenders, even after controlling for several case characteristics, including severity of the crime, criminal history, and type of defense counsel, in time periods nearer to filing deadlines and general election dates. By handing down longer sentences to minority defendants, elected judges may be responding not only to their own implicit bias but also in many cases to clear signals from the public that it wants the justice system to adopt a more punitive approach to sentencing convicted criminals (Enns 2014, 2016), especially when they are Black (e.g., Park 2017).

It follows that if Black defendants receive longer sentences from elected judges, and are sentenced more frequently to incarceration, than White defendants, one long-term consequence is that Black inmates will comprise a larger proportion of the state prison

population than White inmates. Thus, I propose H3, the *Elections Racial Disparity Hypothesis:*

> States with elections for retaining trial judges will have higher Black-to-White incarceration disparities than states without such elections;

and H4, the *Non-Partisan Elections Racial Disparity Hypothesis*:

> States in which trial judges are re-elected in non-partisan elections will have higher Black-to-White incarceration disparities than states with partisan judicial elections.

Of course, it is important to note that there is a distinction between disparities in the criminal justice system and actual discrimination exhibited by one individual toward another or toward a class of persons. Disparities in the system may well result from discriminatory behavior by judges, or from myriad other factors, but uncovering discrimination requires case-level data and is beyond the scope of this project. Regardless of the antecedents of disparities, however, the disparity is itself important to study and understand, even if the greater understanding leads to no more certainty about the extent to which observable disparities involve bias and discrimination.

Responsiveness to Public Opinion

The foregoing hypotheses assume that elected judges are more punitive because they are more responsiveness to public opinion, which is also relatively punitive. In the dataset that I introduce in chapter 3, the range for the percentage of the public offering the punitive response to public opinion polling is .44 to .67, showing that even the least punitive states have large pluralities exhibiting punitive attitudes. Previous research has demonstrated that incarceration rates increased when the public's overall mood became

more punitive (Enns 2014, 2016).[7] Following this previous research, I propose H5, the *Public Opinion Hypotheses*:

> States where a higher share of the population expresses punitive attitudes toward crime and punishment will have higher levels of criminal justice outcomes than states with lower levels of the public expressing punitive attitudes.

Trial judges who must face a punitive public to retain their seats on the bench are likely to be at least partly responsible for rising incarceration rates, but the impact of elections is likely to depend in large part on the state's level of punitiveness.[8] Thus, while the level of public punitiveness increases punitive policy outcomes, the impact of public opinion should be conditioned by the presence or absence of judicial elections in the states. In states where a smaller share of the public expresses punitive attitudes toward crime, the presence of elections should facilitate lower levels of punitive policy outcomes than states where a greater share of the population expresses punitive attitudes.

Punitive mood and retention methods likely interact to influence incarceration rates in a state. In states with elections to retain trial judges, punitive mood should shape the outcomes of interest while public opinion should have a much smaller impact on incarceration in states where judges are not retained by popular election. Figure 2-1 represents a hypothetical effect of mood on criminal justice outcomes, where, as punitive

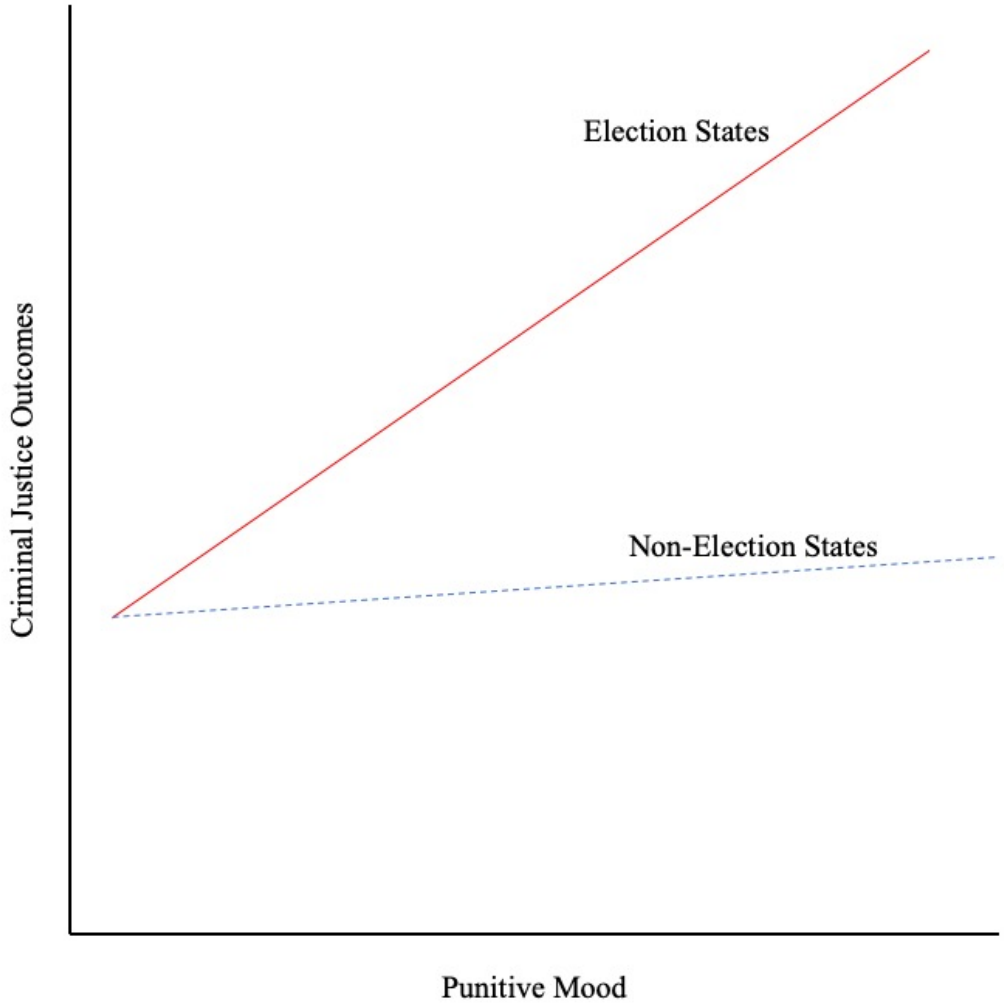
[7] In chapter 3, I use the NPS dataset and the newly constructed Punitive Attitudes Dataset for the States to replicate one of Enns' studies to validate both the reliability of his results using different data and to demonstrate the validity of the PADS data.

[8] While higher levels of punitive mood should be associated with higher levels of punitive policy, especially in states with trial court elections, even the least punitive states have large pluralities exhibiting punitive attitudes. In the dataset introduced in chapter 3, the range for the percentage of the public offering the punitive response to public opinion polling is .44 to .67.

mood increases in election states, criminal justice outcomes should also increase, while in non-election states, outcomes remain relatively stable despite punitive mood.

Partisan elections in low-punitive-mood states provide voters a clear cue about the partisanship of an incumbent judicial candidate, giving voters the opportunity to vote for a candidate with more liberal views, i.e., one who will be less punitive and more racially equitable. Conversely, partisan election states with high public punitiveness should have the necessary heuristic to vote for a more conservative, i.e., presumably punitive, judge.

Figure 2-1: Hypothetical Effect of Mood on Criminal Justice Policy Outcomes



In non-partisan states (as well as in states with uncontested retention elections), judges should use their campaigns to communicate their position on crime and their judicial history in sentencing criminal defendants. Voters, then, use this retrospective accounting to select judges who match their own punitive preferences. So, states with non-partisan elections and low levels of public punitiveness should have lower levels of punitive policy outcomes in response to the campaign signals sent by the judicial candidates rather than responding to the party heuristic provided on the ballot. On the other hand, high-public-punitiveness states should select judges who have demonstrated a judicial history of "lockin' 'em up." Thus, I propose H6, the *Public Opinion Interaction Incarceration Hypothesis:*

> The impact of public punitiveness on incarceration rates should vary across different selection systems, with mood having a greater impact on incarceration for judicial election systems and a lesser impact on non-election systems;

H7, the *Non-Partisan Election Public Opinion Interaction Incarceration Hypothesis*:

> The impact of public punitiveness on incarceration rates should vary between states with non-partisan elections and states with partisan elections, with non-partisan states exhibiting more responsiveness to public opinion than partisan states;

H8, the *Public Opinion Interaction Racial Disparity Hypothesis*:

> The impact of public punitiveness on racial disparity should vary across different selection systems, with mood having a greater impact on racial disparities for judicial election systems and a lesser impact on non-election systems;

and H9, the *Non-Partisan Election Public Opinion Interaction Racial Disparity Hypothesis:*

The impact of public punitiveness on racial disparity should vary between states with non-partisan elections and states with partisan elections, with non-partisan states exhibiting more responsiveness to public opinion than partisan states.

Table 2-1 summarizes the foregoing hypotheses, including the hypothesis name, the dependent variable used to test the hypothesis, and the expectations for each retention mechanism.

Table 2-1: Summary of Hypotheses and Expectations

Hypothesis	Dependent Variable	[Baseline] Expected β
Elections & Public Opinion Main Effect		
H1. Elections Incarceration Hypothesis	Incarceration Rate	[No Elections] URE, NPE, PE: +
H2. Non-Partisan Elections Incarceration Hypothesis	Incarceration Rate	[Partisan Elections] NPE: +
H3. Elections Racial Disparity Hypothesis	Black Disparity	[No Elections] URE, NPE, PE: +
H4. Non-Partisan Elections Racial Disparity Hypothesis	Black Disparity	[Partisan Elections] NPE: +
H5. Public Opinion Hypothesis	Incarceration Rate Black Disparity	Public Punitiveness: +
Elections & Public Opinion Interactions		
H6. Public Opinion Interaction Incarceration Rate Hypothesis	Incarceration Rate	[Low Punitive NE] Punitiveness x URE: + Punitiveness x NPE: + Punitiveness x PE: +
H7. Non-Partisan Election Public Opinion Interaction Incarceration Hypothesis	Incarceration Rate	[Low Punitive PE] Punitiveness x NPE: +
H8. Public Opinion Interaction Racial Disparity Hypothesis	Black Disparity	[Low Punitive NE] Punitiveness x URE: + Punitiveness x NPE: + Punitiveness x PE: +
H9. Non-Partisan Election Public Opinion Interaction Racial Disparity Hypothesis	Black Disparity	[Low Punitive PE] Punitiveness x NPE: +

Note: NE = No Elections; URE = Uncontested Retention Elections; NPE = Non-Partisan Elections; PE= Partisan Elections. For each hypothesis, the baseline condition of the independent variable, *Retention Method*, is listed in brackets. Each of the election types below the bracketed baseline is compared to that baseline condition.

Data & Measures

I use data from several sources to test the above hypotheses. The primary dataset is the "National Prisoner Statistics, 1978-2015" (NPS) dataset compiled annually by the US Department of Justice: Office of Justice Programs, Bureau of Justice Statistics (Bureau of Justice Statistics 2017). The NPS is an annual census of state prison systems conducted by the US Department of Justice, Bureau of Justice Statistics. The dataset is an annual census of state prison systems, including the number of inmates incarcerated in the system, broken down by inmate race, sex, health condition, and so forth, as well as the number of newly admitted prisoners in a given year. The dataset has been widely used in disciplines such as public health, sociology, and criminology, but has not been used in political science or the judicial politics literature. The dataset contains variables measuring state prison populations for every state plus D.C. over a 38-year period, from 1978 through 2015 (N=1938 state-years).

Dependent Variables

The unit of observation is the state-year. From the data in the NPS, I construct two dependent variables, both of which are continuous measures, for each state-year:

- *Change in Incarceration Rate*: Based on the total population of a state's prison system in a given year, the incarceration rate is standardized by dividing the prison population by the state population per 100,000. Because a state's incarceration rate is a function of its sentencing rate, *Incarceration Rate* can also be attributed to judicial decisions. And because incarceration rates are responsive to public opinion, they are an appropriate measure of punitiveness to capture judicial responsiveness

to public opinion. The dependent variable for each state-year observation is the change in the incarceration rate from the prior year.

- *Change in Black Disparity*: This variable captures the ratio of Black inmates in a state's prison system to the number of White inmates. Consistent with the literature, this variable is also standardized by population. The Black prison population is divided by the state's Black population per 100,000. Similarly, the White prison population is divided by the state's White population per 100,000. The Black incarceration rate is then divided by the White incarceration rate. The resulting quotient is *Black Disparity,* as in Equation 2.1 (Blumstein 1982, 1993; Bridges and Crutchfield 1988; Yates 1997; Yates and Fording 2005).

$$\textit{Black Disparity} = \frac{\text{(Black Prison Population / State Black Population 100,000)}}{\text{(White Prison Population / State White Population 100,000)}} \qquad [2.1]$$

As an example, in Oklahoma in 1983, the White incarceration population was 4,842; the White population in 100,000s was 25.98 (i.e., 2,598,000 White residents); the Black prison population was 2,057; and the Black population in 100,000s was 2.05 (i.e., 205,000 Black residents). The resulting *Black Disparity* is 5.38, which can be calculated using Equation 2.2.

$$\textit{Black Disparity}\ (\text{OK83}) = \frac{(2{,}057 / 2.05)}{(4{,}842 / 25.98)} = \frac{1{,}003.41}{186.37} = 5.38 \qquad [2.2]$$

In other words, in Oklahoma in 1983, for every White prisoner there were more than five Black prisoners, in proportion to their populations in the state. The dependent variable for each state-year observation is the change in the Black disparity from the prior year.

I will discuss these dependent variables in more detail in chapters when they are used in the analysis by showing both national and state-by-state trends in each variable over time.

Independent Variables

The primary independent measure is *Retention Method*. Goelzhauser (2018) advises to distinguish between selection methods, i.e., the mechanism by which judges are first seated, and retention methods, i.e., how judges retain their seat on the bench (see also Goelzhauser 2016; Kritzer 2016). I use the retention mechanism in my empirical analysis because my theory posits that a trial judge's sentencing behavior is designed, in part, to respond to the public's mood to ensure the judge's reelection in the future. Thus, knowing (s)he must face the voters to remain a judge, an incumbent is likely to, either consciously or not, alter their behavior on the bench.

Retention Method is a categorical variable coded as 0 for states where trial court judges do not face election by the public to retain their seat on the bench, coded 1 for states with uncontested retention elections, coded 2 for states with non-partisan elections, and 3 for states with partisan elections. In the analysis, *Retention Method* is specified as a categorical variable, which is tantamount to including a series of dummy (indicator) variables for each type of election, with no elections as the reference category. Thus, the coefficients for *Uncontested Retention Elections*, *Non-Partisan Elections*, and *Partisan Elections* indicate the difference between no elections and the other three types of retention elections. During the time-series, six states changed the mechanism by which trial court judges were retained, and since the unit of observation is the state-year, these changes are captured in the dataset.

In addition to the mechanism of judicial retention, I am also interested in the effect of public punitiveness on punitive outcomes in the states over time. To account for punitive mood in the states, I include the variable *Public Punitiveness* from the dataset introduced in chapter 3, lagged three years because previous research has shown that a three-year lag in public opinion has a significant impact on incarceration rates (Enns 2014, 2016), a result I confirm in chapter 3. Essentially, this variable is a measure of public opinion on crime and punishment that is based on public opinion polling and varies by state-year. The MRP method used to construct state public mood is described in more detail in chapter 3. I also include a multiplicative term (i.e., formed by multiplying the retention mechanism times *Public Punitiveness*) to test the interactive hypotheses, Hypotheses 5 and 6, that states with elected judges will be more responsive to the punitive mood than in states without judicial elections.

In all analyses, I control for variables that have been shown to be related to the dependent variable or could, *a priori*, impact the outcome of interest. The control variables fall into one of four categories: Political, legal, geographic-demographic, or economic characteristics of the states, as described below.

Political Factors

Criminal justice policy takes place in an inherently political environment (Boldt and Boyd 2018; Brace and Hall 1995; Fording 2018; Gottschalk 2009; Yates and Fording 2005; but see Ewald 2012). And besides the presence or absence of judicial elections, other political features of state judiciaries might also account for the variation in judicial behavior that produces more punitive policy outcomes. The amount of time remaining in a judge's term has been shown to influence their behavior (Huber and Gordon 2004), with judges

handing down longer sentences in election years than in non-election years. However, the many judges facing re-election in the states are often elected at different times. While the length of time before the election would capture this important variation at the judge level, it is unfortunately unavailable at the state level. However, a suitable proxy measure exists. Regardless of when a judge is selected, there is a set number of years until their next election, and that interval is the same for all trial judges across the state. To account for the temporal nature of judicial terms, I control for *Judicial Term Length*, expecting that as the term length increases, thus providing more independence from the public, punitive outcomes should decrease.

On the other hand, judges might be responsive in the middle of their terms if the public can recall them. Similar to opposition to judicial elections, judicial recall was a reform promoted by the Progressives, and similar to reforms such as non-partisan elections and merit plans, it was a reform that was slow to catch on (Shugerman 2012). Some states permit voters to recall judges via popular referenda, while others do not. To account for this alternative political threat, I use a dummy variable, *Recall*, that takes the value of 1 if the state permits judicial recall by voters, and is coded 0 otherwise.

Moreover, prosecutors make several important decisions in the course of a case from the decision to pursue or dismiss charges to making pre-trial plea agreements with defendants and their lawyers. These decisions certainly reduce judicial discretion (Spohn 2009, 66). However, whether or not prosecutorial decisions are made on the basis of race is a matter of dispute in the literature (Walker et al. 2012, 219). On the one hand, some studies do find some evidence of racial disparities in some prosecutorial decisions, such as filing felony rather than misdemeanor charges (Crutchfield et al. 1995), the decision to

sentence under mandatory minimum sentences (Bjerk 2005; Farrell 2003), or to sentence defendants as habitual offenders (Crawford 2000; Crawford et al. 1998). On the other hand, some studies have uncovered no effect of race on prosecutorial decisions. Franklin (2010), for example, finds that race had little to no impact on the decision of prosecutors to drop charges in a nationally representative sample of nearly 5,000 state felony drug cases. No race-based effects have been found in other, arguably more serious cases, such as sexual assault (Beichner and Spohn 2005; Spohn and Holleran 2001). However, prosecutors cannot completely bind the judge's hands on sentencing cases where charges have been filed. Judges have the power to reject plea agreements, for example, if they do not like the sentencing terms of the agreement.

Elected prosecutors are, for the same reasons as elected judges, likely more responsive to the same political forces as elected judges when it comes to criminal justice matters than unelected prosecutors. First, previous research has demonstrated that elected prosecutors are responsive to public opinion (Wright 2012). Nelson (2014), for example, demonstrates that, following a public referendum on the legalization of marijuana, prosecutors in Colorado were more lenient on marijuana possession defendants in localities with higher levels of support for the referendum. Second, prosecutors alter their behavior with respect to plea bargaining when electoral considerations are relevant. For example, Bandyopadhyay and McCannon (2014) demonstrate that elected prosecutors in North Carolina were more likely to take a case to trial (and, relatedly, less likely to plea bargain with defendants) in an election year, an effect that was greater if the prosecutor was facing a challenger (Bandyopadhyay and McCannon 2015; McCannon 2013). However, not all states elect their chief prosecutors (Coppolo 2003). To capture this variation and the

consequences it likely produces, I include an indicator variable, *Elected Prosecutor*, that takes a value of 1 if the state elects its chief prosecutor and 0 otherwise.

Legal Factors

Hall and Windett (2015) further report that judicial influence over incarceration rates is higher in states that do not have an intermediate appellate court (IAC). I capture the states without an IAC with an indicator variable, *No IAC*, which takes a value of 1 if the state does not have an IAC and a 0 otherwise.[9]

Police also play a role in shaping the criminal justice system. After a crime has been committed or alleged, police must make an arrest. Thus, the *Arrest Rate* and *Arrest Disparity* are likely to impact the carceral population and disparate outcomes, so I control for them.

Geographic and Demographic Factors

Because of the South's special history of punitiveness and racial bias (Acharya et al. 2016), southern states are identified by the indicator variable, *South.* Rather than using the US Census regional designation, which includes states like Maryland and Delaware in the South, southern states are identified in my study as those 11 states that belonged to the Confederacy during the Civil War (Valentino and Sears 2005).[10]

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[9] The ten states with no intermediate appellate courts are Delaware, Maine, Montana, Nevada, New Hampshire, Rhode Island, South Dakota, Vermont, West Virginia, and Wyoming. Voters in Nevada approved a ballot measure creating an IAC during that state's November 2014 election. The newly formed IAC was formally empaneled beginning January 1, 2015, but, according to media reports, did not begin operating until later that year (Whaley 2014). Since 2015, the year the Nevada IAC began operation, is the final year in the dataset, I leave that observation coded as 1 for No IAC, although the results are unchanged if the observation is coded to 0.

[10] The impact of additional demographic factors is explored in the appendix (c.f. tables A4-4 and A5-5).

Demographic characteristics of the states, measured with US Census Bureau estimates for each year, are also included in the analysis. *Percent Black* measures the percentage of the population in the state that is Black. I do not have a clear expectation for this variable. On one hand, a higher percentage of Blacks in the population could represent a racial threat to the White majority population (Key 1949), leading to a higher incarceration rate (but see Acharya et al. 2016; Voss 1996). On the other hand, it is also possible that a greater proportion of Blacks in the state could lead to greater inter-racial contact (Allport 1958; Stein et al. 2000), reducing racial threat as well as racial disparity in state prisons. Because previous research has shown that higher Black populations are associated with reduced racial disparity (Bridges and Crutchfield 1988; Yates and Fording 2005), I expect a negative coefficient. However, the combined logic of the previous studies suggests a possible curvilinear effect of the Black population on the outcomes, so I control for *Percent Black*^{2} and expect a positive coefficient on this quadratic term.

Percent Bachelor's Degree measures the percent of a state's population with a bachelor's degree or higher. I expect this variable to reduce racial disparity as individuals who have attained higher levels of education are more likely to have been exposed to people of color, lessening the possibility of racial threat (Oliver and Mendelberg 2000; Short 1993), and because uneducated young men—especially those lacking a college degree—are more likely to be incarcerated than more highly educated individuals (National Research Council 2014).

Economic Factors

Some previous research on incarceration rates has shown that prison population is affected by economic conditions (Rosenfeld 2009; Rosenfeld and Fornango 2007). Social

theorists argue "that the poor are punished most because their involvement in crime and life circumstances are seen as threatening to the social order" (National Research Council 2014, 127). Therefore, as the income per capita of a state increases, we should expect to witness a decline in punitive policy outcomes. To account for this, I control for *Income Per Capita*.

Table 2-2 lists each independent variable and control variable detailed above along with the direction of the relationship I expect it to have with the dependent variables and the source of the data. Table 2-3 reports descriptive statistics for each variable, including its mean value, other relevant measure of central tendency, or the frequency of 1 in the case of indicator variables; the standard deviation when relevant; and the range when relevant.[11]

[11] Additional controls are estimated in robustness checks in the appendix (c.f. tables A4-4 and A5-5).

Table 2-2: Independent and Control Variable Expectations and Data Sources, States

	Expected β	Data Source
Uncontested Retention Elections	+	National Center for State Courts http://www.judicialselection.us/
Non-Partisan Elections	+	National Center for State Courts http://www.judicialselection.us/
Partisan Elections	+	National Center for State Courts http://www.judicialselection.us/
Public Punitiveness	+	Punitive Attitudes Dataset for the States Chapter 3 of this Dissertation
Judicial Term Length	–	National Center for State Courts http://www.judicialselection.us/
Recall (of Judges)	+	National Center for State Courts http://www.judicialselection.us/
No IAC (Intermediate Appellate Court)	+	Ballotpedia.org & NCSC https://ballotpedia.org/Intermediate_appellate_courts
Arrest Rate & Arrest Disparity	+	Federal Bureau of Investigation https://crime-data-explorer.fr.cloud.gov/
Elected Prosecutor	+	Coppolo (2003)
South	+	Former Confederate States Valentino and Sears (2005)
Percent Black	–	US Census Bureau Statistical Abstracts https://www.census.gov/library/publications/time-series/statistical_abstracts.html
Percent Bachelor's Degrees	–	US Census Bureau Statistical Abstracts https://www.census.gov/library/publications/time-series/statistical_abstracts.html
Income Per Capita	–	US Census Bureau Statistical Abstracts https://www.census.gov/library/publications/time-series/statistical_abstracts.html

Table 2-3: Descriptive Statistics

	Mean or Proportion	Overall SD	Between SD	Within SD
Dependent Variables				
Δ Incarceration Rate	–5.64	43.081	6.133	–15.493
Δ Black Disparity	–0.07	1.633	0.121	1.630
Independent Variables				
Retention Method				
No Elections	0.24	—	—	—
Uncontested Retention	0.16	—	—	—
Non-Partisan Elections	0.40	—	—	—
Partisan Elections	0.20	—	—	—
Public Punitiveness$_{t-3}$	0.58	0.035	0.015	0.031
Political Factors				
Judicial Term Length	12.35	19.425	19.613	0
Recall	0.16	—	—	—
Elected Prosecutor	0.92	—	—	—
Legal Factors				
No IAC	0.20	—	—	—
Arrest Rate$_{t-1}$	4098	1708.119	1122.663	1300.136
Arrest Disparity$_{t-1}$	3.589	1.986	1.336	1.471
Geographic–Demographic Factors				
South	0.22	—	—	—
Percent Black	10.65	11.926	11.976	1.243
Percent Bachelor's Degrees	22.07	7.717	2.937	7.148
Economic Factors				
Income Per Capita	56.26	1027.285	167.414	1013.816

Methods

Randomized experiments are the gold-standard of research design. Such an analysis allows researchers to randomly assign participants to various conditions resulting in a difference in means on some outcome of interest between the control group and one or more experimental groups that can then be attributed to the treatment. Such an experiment

is not only infeasible in studying criminal justice policy (e.g., a researcher cannot assign some random group of participants to commit the same crime in jurisdictions with different judicial retention mechanisms), but it is ethically prohibited. Thus, researchers must rely on various statistical methods to best approximate the relationships of interest.

One approach to the use of statistical methods is to employ a multiplicity of research methods to answer the same question in an effort to establish not only a correlation between the variables of interest, while holding other factors constant, but also attempting to establish a causal relationship. That is the approach I use in this project. I begin by testing my hypotheses with multiple regression analyses to explore support for my hypotheses. I then take advantage of institutional design reforms—i.e., a change from partisan to non-partisan elections—to test for a change in the outcomes of interest following the reforms using difference-in-differences analysis (Angrist and Pischke 2009; Card and Krueger 1993).

Multilevel Regression and Post-Stratification

One problem with prior research on mass punitive attitudes is that the measures do not vary sufficiently. Some studies measure punitive mood over time at the national level, but those measures do not vary by state (e.g., Ramirez 2013), while others measure state-level punitive mood but aggregate temporally, so the state-level measures do not vary from year to year (e.g., Enns 2014). To remedy this problem, I first locate available national public opinion polls in which respondents are queried about their preferences for criminal punishment, prevention, or rehabilitation. Relying on these polling data, I employ multilevel regression and post-stratification (MRP) (Gelman and Little 1997), which has been used to reliably estimate state-level public opinion on other policy issues (see, e.g.,

Enns and Koch 2013; Lax and Phillips 2009, 2012) to develop reliable state-level measures of public punitiveness for years from 1981 through 2016.

Time-Series Analysis

Using the data described, I test the foregoing hypotheses using several different methodological approaches. The primary method is the use of linear time-series regression with panel-corrected standard errors (PCSE) to account for the longitudinal nature of the data (Beck and Katz 1995). Linear regression is appropriate because each of the dependent variables is continuous and normally distributed, but OLS is inappropriate due to concerns about heteroskedasticity and autocorrelation.

Difference-in-Differences Analysis

A properly-specified linear model can provide strong correlational support for the hypothesized relationships. However, the data permit me to take the analysis further in an effort to establish causal evidence in support for some of my hypotheses. As previously discussed, in the time-series of the dataset, six states changed the mechanism by which they retain judges. New Mexico changed from partisan elections to uncontested retention elections, while Utah changed from non-partisan elections to uncontested retention elections. Investigating these two states for causality is beyond the scope of this project.

However, four states—Arkansas, Georgia, Mississippi, and North Carolina—all changed their retention mechanisms from partisan elections to non-partisan elections at varying points in the time series. These institutional design changes provide the opportunity to estimate difference-in-differences models to establish causal support for the various *Non-Partisan* hypotheses detailed above (Angrist and Pischke 2009). To formalize the causal tests, I offer the following hypotheses:

Judicial Reform Incarceration Hypothesis (H10): When a state reforms its judicial retention mechanism from partisan elections to non-partisan elections, the state should exhibit a higher incarceration rate.

and the

Judicial Reform Racial Disparity Hypothesis (H11): When a state reforms its judicial retention mechanism from partisan elections to non-partisan elections, the state should exhibit an increase in racial disparities in incarceration rates.

Table 2-4: Summary of Reform Hypotheses and Expectations

Hypothesis	Dependent Variable	[Baseline] Expected β
H10. Judicial Reform Incarceration Hypothesis	Incarceration Rate	Post-Reform: +
H11. Judicial Reform Racial Disparity Hypothesis	Black Disparity	Post-Reform: +

Conclusion

In this chapter, I presented a democratic theory of judicial behavior. In summary, the theory contends that elected judges should be more responsive to public opinion than judges who do not face re-election. At the state level, electoral consideration faced by judges should translate into greater levels of punitive policy outcomes, especially as the level of punitive mood among the public rises. From that theory, I proposed nine testable hypotheses that explore the relationship between judicial retention mechanisms and state-level policy outcomes. Specifically, I hypothesize about whether judicial elections are associated with—both correlated and causally—annual changes in incarceration and racial disparities in prison populations. In addition, I hypothesize that the relationship between judicial retention mechanisms and these policy outcomes is conditional on the level of

punitive mood. In the next chapter, I introduce the dynamic measures of punitive attitudes in the states that will be used to test the conditionality of the relationship. The following two chapters will test the nine hypotheses posited above.

Chapter 3: A Temporally and Spatially Dynamic Measure of State Punitive Mood

Introduction

In chapter 2, I developed a democratic theory of judicial behavior and proposed several hypotheses to test that theory at the state level. Specifically, I posited that in states where trial judges stand for election to retain their seat on the bench, punitive policy outcomes would be conditioned by punitive mood among the public. Criminal justice policy takes place in a political environment (Boldt and Boyd 2018; Brace and Hall 1995; Fording 2018; Gottschalk 2009; Yates and Fording 2005; but see Ewald 2012), and previous research has shown that legislators, governors, judges, as well as candidates for these offices are responsive to citizens' opinions on punishment (Peffley and Hurwitz 2010). Thus, a comprehensive understanding of Americans' attitudes toward crime and criminal punishment is essential to properly evaluate the criminal justice policy environment.

For reasons extrapolated in chapter 2, measures of public opinion on crime and punishment should be dynamic, rather than static, on both of two dimensions: spatially (across states) and temporally (over-time). However, as the discussion in chapter 2 illuminated, such data do not currently exist. Spatially dynamic measures, such as Lax and Phillips (2012), do not include a temporal dynamism, while temporally varying measures, such as Enns (2014) and Ramirez (2013) do not vary across space. Given this lack of a spatially and temporally dynamic public opinion on crime and punishment, a new measure of punitive attitudes is necessary.

In this chapter, I provide a measure of punitive mood in the states that varies both spatially and temporally. I do so by introducing a new dataset: The Punitive Attitudes

Dataset for the States, or PADS, which contains measures of public punitive mood—evaluated using public opinion polling, multilevel regression and post-stratification, and recursive analysis—for nearly every state from 1981-2016. This dataset overcomes previous gaps in understanding punitive attitudes in the states, specifically that previous data do not vary adequately along one of two dimensions: time and space. Previous measures of punitiveness that vary across time are measured at the national level, failing to account for differences from state to state, while measures that vary from state to state are pooled across time without varying from year to year. Data contained in the PADS vary on both dimensions and will facilitate a research program that investigates punitive opinions by providing scholars with the necessary resources to explore the antecedents and consequences of punitiveness in the American states.

I begin this chapter with a discussion of my data collection procedures and the estimation techniques I use to develop temporally and spatially dynamic measures of public punitiveness. Following that, I validate the data contained in PADS by replicating a previous study investigating the relationship between punitive attitudes and incarceration rates. I conclude with a discussion of research programs that stand to benefit from data contained in PADS and how these data are used in the chapters that follow this one.

Data Collection

Typical indexes in scholarly research that are built from public opinion include multiple questions on the same survey that theoretically tap a single underlying sentiment. The degree to which an item loads onto a broader construct can be assessed in a number of ways, such as exploratory or confirmatory factor analysis, Cronbach's alpha or Item Response Theory (IRT). Because the typical measurement strategy requires the same items

to be asked in all the surveys, it is impractical for time-series analysis of changing attitudes over time.

In this project, I use one of the more popular empirical methods based on Stimson's (1999) measurement of public mood in order to obtain state-level measures of punitive mood that vary annually. The first step in this method is to identify a large number of accessible national surveys that include one or more items tapping support for punitive approaches to dealing with crime. Specifically, I followed Stimson (1999) and Ramirez (2013) and identified relevant surveys archived at ICPSR at the University of Michigan, which serves as a repository of data from scholarly, media, and other sources, in addition to the data of interest here—i.e., individual-level responses on a multitude of public opinion surveys. Following Ramirez (2013), I searched the ICPSR database using the search terms: "capital punishment", "court", "crime", "criminal", "death penalty", "drug", "enforcement", "judge", "judicial", "law enforcement", "marijuana", "police", "prison", "sentence", "sentencing", "spend", and "three strikes." Table 3-1 reports the name of the survey item, survey house, and the how many times a given item appeared in a survey for items collected as a result of these searches.[1]

[1] Some items contain fewer observations than others, which could have implications for the reliability of that item in constructing the index. However, as Stimson (1999, 69) notes, some sampling fluctuation is to be expected in survey research, and the issue of this fluctuation is remedied by the algorithm's smoothing function—"in effect smoothing over time when smoothing across issues is insufficient."

Table 3-1: Survey Items

Survey Item	Survey House	No. of Surveys	Commonality with Index
Cocaine Legalization	ABC	2	–1.000
Favor/Oppose Death Penalty	ABC, CBS, Pew, ANES	23	0.773
Circumstances Death Penalty Justified	CBS, Pew	3	0.987
Death Penalty Deters Crime	ABC, CBS	3	0.995
Death Penalty Deter Terrorism	CBS, Pew	2	1.000
Police Hold Suspect Before Charging	CBS	2	1.000
Treat Juveniles Same as Adults	CBS	3	1.000
Try Juveniles in Adult Court	CBS	3	–1.000
Courts are Too Lenient	ABC	3	0.157
Penalty for Murder – Life or Death	ABC, CBS	5	0.882
Penalty for Murder – Prison, Life, or Death	CBS	4	–0.862
More Police Reduce Crime	ABC	2	–1.000
Marijuana Legalization	ABC, CBS	5	0.999
Legal for Adult to Purchase Marijuana	ABC, CBS	11	0.981
Legalize Marijuana for Personal Use	ABC	3	–1.000
Purpose of Prisons	ABC, CBS	2	–1.000
Shorter Sentences	ABC	2	–1.000
How to Solve Crime	CBS	2	1.000
How to Solve Drug Problem	ABC	2	–1.000
Crime Budget	ANES	14	0.721
Reduce Crime Budget	ABC, CBS	2	1.000
Cuts to Crime Budget	ABC	2	1.000
Drug Prevention Budget	ABC, CBS	5	0.994
Police Search without Warrant	Pew	3	–0.825

Note: Table A3-1 in the appendix contains question wording for each item. Eigenvalue: 1.91 out of a possible 2.42. Common variance explained: 78.99%.

In order to be included in the master data file, the original survey data had to include the respondents' state of residence, sex, and race because these variables would be used to conduct the MRP analysis. After locating a relevant item in a survey, I inspected the source data to make sure it included the data necessary to construct a measure of punitiveness over time and across states. Satisfactory data were compiled into a master data file. Additional demographic and political variables, such as age, ideology, and party identification, were collected where available.

Data were then transformed and new variables on common scales were created in the master data file. Transformations were completed to ensure individual respondents are comparable, even when the survey collected their information differently. For example, survey houses measured educational attainment in different ways. Some used a single category for college graduates, including those who have earned an advanced degree, while other surveys had separate categories for college graduates, master's degree earners, professional degree holders, etc. Thus, for comparability to other surveys with college-degree-and-higher classifications, I collapsed the "advanced degree" categories with the "college graduate" category. Similar transformations were made for religion, income, and partisanship. Table 3-2 reports summary statistics for each variable collected.

Table 3-2: Summary Statistics of Individual Level Variables

Variable	Mean or Proportion	Standard Deviation	Min	Max	Missing
Age	46.6	18.44	12	100	34,274
Education – No HS	.10	—	—	—	14,728
Education – HS Grad	.24	—	—	—	14,728
Education – Some College	.26	—	—	—	14,728
Education – College Grad or Higher	.31	—	—	—	14,728
Income – Low	.29	—	—	—	14,918
Income – Middle	.43	—	—	—	14,918
Income – High	.14	—	—	—	14,918
Race – White	.76	—	—	—	13,011
Race – Black	.13	—	—	—	13,011
Hispanic	.06	—	0	1	44,660
Female	.51	—	0	1	4,229
Male	.46	—	0	1	4,229
Democrat	.40	—	—	—	10,218
Republican	.33	—	—	—	10,218
Independent/Other	.18	—	—	—	10,218
Religion – Protestant	.35	—	—	—	59,176
Religion – Catholic	.16	—	—	—	59,176
Religion – Jewish	.02	—	—	—	59,176
Religion – None/Other	.12	—	—	—	59,176

Note: Missing values are out of 170,438 total respondents across the 88 surveys and 35 years.

Once the master data file consisting of survey answers from 170,438 respondents was completed, I determined which responses for each item would be coded as punitive versus non-punitive. I completed this task by relying on Ramirez (2013) where possible. For example, the item *death1* asks respondents, "Do you favor or oppose the death penalty for persons convicted of murder?" "Favor" is coded as punitive, while "oppose" is coded as non-punitive. When an item was included in my dataset that Ramirez did not include, I used an intuitive coding scheme. For example, the item, *coke,* was not included in Ramirez's (2013) study. Respondents were asked, "Do you favor or oppose legalizing the possession of small amounts of cocaine for personal use?" Opposition to legalization was considered "punitive," while favoring legalization was considered "not punitive."

I constructed two variables using this data. First, the variable, *Punitive Opinion Holders*, is a dichotomous variable taking on the value of 1 if the respondent offered a punitive response to any one of the punitiveness items and 0 if the respondent offered a non-punitive response. Respondents without an opinion are coded as missing. *Punitive All* takes a value of 1 if the respondent offered a punitive response to one of the items and a 0 if the respondent offered a non-punitive response or a non-response, such as "don't know." The mean values for *Punitive Opinion Holders* and *Punitive All* in the master, respondent-level dataset are 0.66 and 0.59, respectively, indicating that the average respondent leans toward punitive attitudes when dealing with crime.

The master data file, in total, contained responses to 24 items from 88 surveys. Each item in the data file was asked at least twice in two different years, a requirement for the recursive estimation (Stimson 1999). Data span the years 1981 through 2016. During the

time series, only four years exist without data.[2] Such missingness is remedied by the recursive analysis. In sum, there are entries for punitiveness items for 169,953 respondents over the course of the 36 years.

Estimation Techniques

An initial step for estimating the measurement properties of a social science index is often to conduct a confirmatory factor analysis for all the items of all the respondents. In time-series analysis of public opinion data, such a measurement strategy would create a huge missing data problem, since as table 3-1 demonstrates, when using different surveys with different respondents, and even within a single survey not every respondent answered every question, and not every question was asked of every respondent in every survey. This problem is remedied by the use of the dyadic recursion algorithm developed by Stimson (1999) and employed by Ramirez (2013) and Enns (2016) to develop measures of latent punitive mood.

Some have described Stimson's algorithm as a dynamic factor analysis with missing data. Basically, the algorithm solves two problems of missingness. First, at the individual level, it remedies the missing data problem by relying on "what we do know" (Stimson 1999, 133). What we do know is how respondents answered a punitive question at two points in time, and thus we know the relative values of punitiveness. Second, at the macro level, the technique solves the problem of missing years, i.e., years in which I do not have a survey that asks a punitive question. The algorithm solves the missing-years problem by estimating the opinions and how they change from one point in time to another using backward and forward recursion, which imputes an estimate of punitive mood for

[2] The missing years are 1983, 1985, 1987, and 1993.

missing years in the series "based on the weighted average of the ratios that are observable in the data" (Ramirez 2013, 340). Thus, a measure of punitiveness is estimated for each year in the series, including for the years without survey data.[3]

In the process, the Stimson algorithm produces two important products. First, the algorithm estimates the association between each issue and the latent variable as in a factor analysis. As table 3-1 reports, all but one of the 24 items load onto a single dimension with a value greater than 0.70. Only *lenient* has a low loading value (0.16).[4] These loading values are effectively the correlation between the survey item and a latent mood, so each survey item, except *lenient,* is correlated at a 0.70 level or greater with an underlying attitude. These results suggest that each of the items moves together with a single underlying mood. Together, the 24 items scale onto a single latent dimension with an eigenvalue of 1.91, indicating they are tapping a single underlying common sentiment that can appropriately be used to create an index of punitive attitudes over time.

Figure 3-1 plots four example survey items over time. The selection criteria for inclusion in figure 3-1 are twofold. First, the item must have been included in at least one survey in five different years over the course of the time period under analysis. Repeated

[3] For more technical detail on the dyadic recursion algorithm, see Stimson (1999), appendix 1. Other scholars have used and validated the methodology to estimate other public opinion issues, including public liberalism (Erikson et al. 2002a; Stimson 1999), trust in government (Chanley et al. 2000; Keele 2007), support for Congress (Durr et al. 1997) and the Supreme Court (Durr et al. 2000), social capital (Keele 2005), and racial preferences (Kellstedt 2000).

[4] Although it might seem striking that all of the items, except one, have such high loading factors, especially given the range of commonality reported by other scholars measuring punitive mood (e.g., Enns 2016; Ramirez 2013), to have all survey items correlate so highly with the underlying mood in recursive analyses is not uncommon. For example, Keele (2007, 245) reports correlations ranging from 0.73 to 1.00 on his measures of social capital, while Chanley et al. (2000, 243) report a range on their measure of political trust from 0.83 to 1.00.

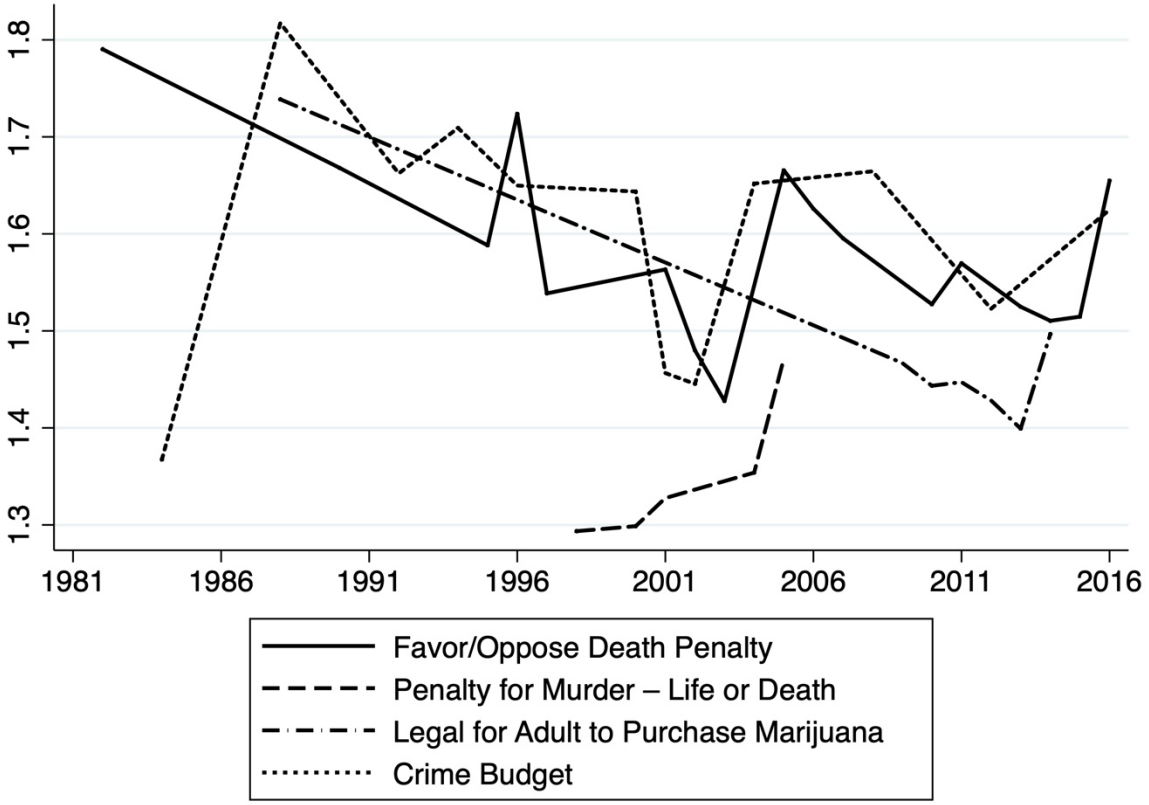
asking of a survey item in multiple years provides a more reliable method of tracking movement in respondents' preferences for that item. Second, the item must have a high loading on the punitiveness index (see table 3-3). Factor loading values provide the correlation between the constituent variable, in this case the survey item, and the underlying latent variable, i.e., punitiveness. Correlations above 0.70 are considered "strong", so that is the threshold I considered when selecting which items to include in figure 3-1 (Mindrila and Balentyne n.d., 9). The four survey items plotted in figure 3-1 are: *death1* (asked in 17 different years, loading value = 0.773); *options2* (5, 0.882); *pot2* (7, 0.981); and *spend1* (11, 0.72).

All four of the variables are set to a common scale (Stimson 1999). Respondents providing a punitive response are set to equal 2, non-punitive responses are valued at 1, and non-responses (e.g., don't know) are set to 0.[5] Thus, higher values represent greater levels of punitiveness. The vertical axis in figure 3-1 reports the mean yearly response rate on this scale for each of the items included. A first glance at figure 3-1 would seem to indicate that the four measures may not be tapping the same latent variable because their means are at different levels. For example, the mean for *options2* is markedly lower than the means for the other items. However, the item's placement in the vertical space is not the critical marker. What the reader should take away from figure 3-1 is the similarity in movement (Stimson 1999). The four items, although asking survey respondents about their preferences on different policies, are moving in a remarkably tandem manner. When

[5] I display the trends somewhat differently than other researchers. Ramirez (2013), for example, reports annual trends in terms of the proportion of respondents offering a punitive response, whereas my trends are on more of an ordinal scale. Despite this coding difference, the trends exhibit strikingly similar patterns. The patterns reported in figure 3-1 are unchanged if I display them as a proportion of punitive responses.

support for the death penalty (*death1*) declines, support for spending on crime (*spend1*) declines alongside it. When support for the death penalty increases, so does opposition to the legalization of marijuana (*pot2*). On visual inspection, it is clear that these variables are measuring an underlying punitive mood, or punitiveness.[6]

Figure 3-1: Four Example Series, 1981-2016

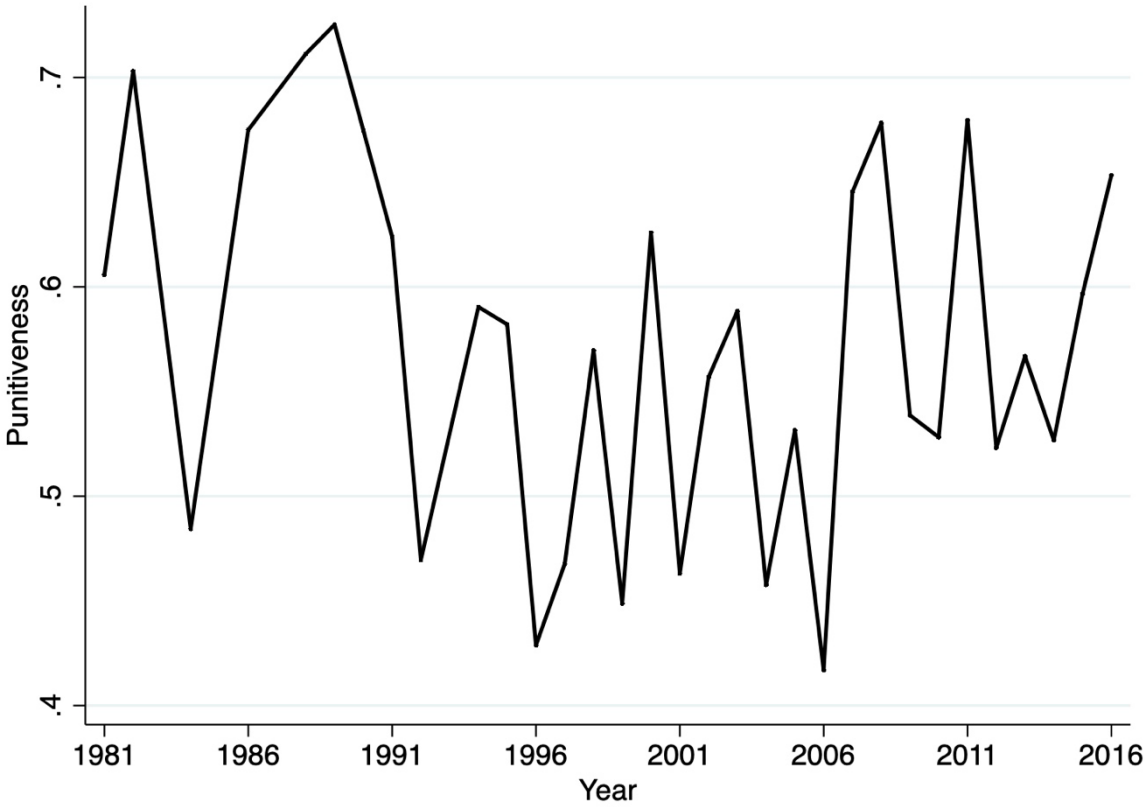


The second, and more important product of the Stimson algorithm is the yearly measure of punitiveness. Figure 3-2 reports the net punitive attitudes across time from 1981 through 2016. The over-time fluctuations in punitive opinions evidenced in figure 3-2

[6] Some of the items contained in the analysis may be viewed as having some racial undertones, hinting at the possibility of separate dimensions of punitiveness—one dealing with "White" crime and one related to "Black" crime. However, an additional analysis using the Stimson method allowing for the items to load on two dimensions revealed that a single dimension is the most appropriate estimation. Table A3-2 in the appendix reports the findings of the two-dimension analysis.

demonstrate the importance of a temporally dynamic measure of punitiveness. Importantly, figure 3-2 also very closely resembles Ramirez's (2013) figure 1 (337), indicating my measure as a gauge of public opinion on punitiveness is facially valid.[7]

Figure 3-2: Punitive Attitudes Across Time, 1981-2016



Note: This figure displays the raw percentage of Americans expressing a punitive opinion in a given year. A figure with the Lowess smoothing function applied is located in the appendix (figure A3-1).

Having demonstrated the validity of the recursive estimate of national-level punitiveness, I develop a measure of state-level punitiveness. I do this by conducting a multi-level regression and post-stratification (MRP) estimation for each year with available

[7] Although figure 3-1 has more annual fluctuation than is present in Ramirez's figure 1, during the time periods during which both Ramirez and I have data, the level of punitiveness fluctuates primarily in the range between 55 and 70%.

data. The first step in the MRP procedure is to estimate a hierarchical model of respondents' stated policy preferences using individual and geographic predictors. The dependent variable in this model is *Punitive All*, which takes on a value of 1 if the respondent provided a punitive response to the survey item and a 0 otherwise. Thus, respondents who offered a non-response (e.g., don't know, not sure, no opinion, etc.) are included in the dataset and coded as 0. Including non-response individuals is critical because MRP estimates state-level opinions based on all residents of a state, not just those residents who hold opinions (Kastellec et al. 2016).

Because of the computational complexity of the MRP analysis, I was able to include only a limited number of variables at each level. Thus, I included the most important likely predictors of punitive attitudes. At the individual level, I included an interaction between the respondent's race-ethnicity and sex, which produced six categories: Black Females, Black Males, Hispanic Females, Hispanic Males, White Females, and White Males. An abundance of research shows that Whites and racial minorities experience the criminal justice system differently and thus have different levels of support for policies dealing with crime (Gibson and Nelson 2018; Hurwitz et al. 2015; Peffley and Hurwitz 2010, 1998; Peffley et al. 2017), so not only are race and sex likely to be among the more important predictors of punitive attitudes, these characteristics are also important for state-level weighting in MRP.

At level 2, the state, I included a variable that measures the percentage of the state that is a racial minority, defined as either Black or Hispanic. Because Whites are a numerical majority in all states and there exists an implicit association linking Blacks with crime (Allport and Postman 1947; Correll et al. 2002; Eberhardt et al. 2004) as well as a

recent association of Hispanics with criminality (Kohut et al. 2006; Sohoni and Sohoni 2014), I expect that as the percentage of each of these two racial-ethnic groups increases, the average respondent in a state will become more punitive in their attitudes toward crime.[8] I also include at level 2 a variable measuring the overall crime rate in a state. Crime rates are an important predictor of punitive attitudes (Ramirez 2013). Consistent with past research, I expect that as crime rates increase, individuals will express more punitive moods. Finally, I include an indicator variable for region to account for regional differences in punitive attitudes across the country (Kastellec et al. 2016).[9] Formally, the multilevel model is estimated using the following equation:

$$\Pr(\text{punitive response}) = \alpha + \delta_i + \sigma_s + \rho + \varepsilon \qquad [3.1]$$

where δ is an interaction between the individual's race and sex, σ is a vector of state-level predictors, ρ is an indicator variable for each region, and ε is the error term.

The second and final step in MRP is post-stratification, or weighting. This step is accomplished by weighting each respondent according to their demographic-geographic profile using census data on actual state populations. By applying such weights, MRP produces "reasonably accurate estimates of state public opinion" for each state (Kastellec et al. 2016, 2).

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[8] I confirm these expectations using a multilevel model and a probit model. In both models, higher percentage of a respondents' state population that is Black predicts a significantly greater probability of the respondent providing a punitive response, consistent with previous research on the racial threat hypothesis (Taylor 1998; but see Acharya et al. 2016; Voss 1996). In the multilevel model, but not in the probit model, a greater share of a respondent's state population that is Hispanic significantly predicts a greater probability of the respondent providing a punitive response. See table A3-3 in the appendix. Taylor (1998) does not report racialized policy attitudes associated with Hispanic population, but her data preceded the rise of the Hispanic-criminality implicit association and the rise of illegal immigration as a wedge issue for Republicans.

[9] Regions included are West, Midwest, South, Northeast, and DC.

I estimated an MRP analysis for each year of survey data available.[10] Such an approach was required to create the data necessary to produce a measure of punitiveness that varies across states and over time. MRP produces spatially dynamic estimates of punitive attitudes; it estimates the level of punitiveness in each state. Conducting this analysis for each year produced the temporally dynamic level of punitiveness. After estimating punitiveness for each state in each year, I applied a Lowess smoothing function (Cleveland 1979) to the data in order to smooth over any large jumps in punitive attitudes that may have resulted from sampling error—i.e., to "eliminate most of the noise induced by sampling fluctuation" (Stimson 1999, 135). The Lowess curve was applied to each state's data separately so that the smoothing function operated over time within a state. The end result of these estimations is a new dataset of punitive mood in the states that varies from state-to-state, year-to-year, and thus state-year-to-state-year.

Exploring the PADS

Before turning to a validation of the measures contained in the PADS, I first explore the measures of punitiveness descriptively. In table 3-3, I report descriptive statistics for *Public Punitiveness* by year. Looking at the third column, which reports the raw national-level mean, one can see clearly the numbers behind jumpiness present in figure 3.2 above.

[10] Enns (2016) uses state-year level measures of punitiveness, in which he estimated punitiveness for some years then applied the Stimson (1999) algorithm to provide estimates for years with missing data. I estimated data using a similar approach, and the estimates are provided in PADS. However, the recursive estimates of punitiveness and the MRP estimates are correlated at only $r = 0.091$ and the correlation between the two Lowess smoothed estimates is only $r = 0.020$. Because the application of the Stimson algorithm to obtain estimates for missing years produces correlations so low, I opt to use the MRP estimates rather than the recursive estimates, even though it reduces the number of years under observation.

Table 3-3: Summary Statistics for Public Punitiveness, by Year

Year	Smoothed Mean	Raw Mean	Standard Deviation	Min	Max
1981	.63	.60	.02	.55	.64
1982	.63	.73	.05	.52	.80
1984	.64	.45	.04	.37	.52
1986	.63	.68	.02	.64	.72
1987	.62	.71	.03	.58	.76
1989	.61	.72	.03	.67	.78
1990	.60	.67	.04	.51	.74
1991	.59	.63	.03	.56	.70
1992	.58	.39	.11	.14	.61
1994	.57	.72	.04	.65	.78
1995	.56	.57	.04	.45	.64
1996	.56	.38	.04	.29	.48
1997	.56	.46	.01	.44	.48
1998	.56	.48	.06	.35	.60
1999	.55	.44	.10	.29	.59
2000	.54	.63	.04	.53	.71
2001	.55	.53	.09	.33	.71
2002	.55	.56	.02	.49	.61
2003	.55	.55	.07	.43	.67
2004	.55	.55	.05	.37	.70
2005	.56	.54	.06	.36	.65
2006	.57	.62	.08	.3	.73
2007	.57	.64	.04	.44	.74
2008	.57	.61	.08	.46	.75
2009	.58	.55	.06	.38	.68
2010	.58	.52	.04	.47	.60
2011	.58	.68	.04	.60	.76
2012	.58	.51	.04	.44.	.59
2013	.56	.57	.04	.46	.63
2014	.59	.53	.07	.34	.65
2015	.59	.60	.08	.35	.77
2016	.59	.65	.05	.51	.74

However, there is no reason to believe the annual changes are so truly characterized by such variation, especially when aggregating to the national level. Thus, the second column reports the smoothed national level mean. This Lowess-smoothed mean shows that from the early 1980s, punitive attitudes among the mass public dropped fairly steadily and consistently throughout the 1990s and the early part of the 2000s. Around 2005, punitive

attitudes began ticking up slowly, but, as of 2016, still had not reached the high levels observed in the 1980s.

Of course, this national-level aggregation obscures the state-level variation. Table 3-4 reports the ten least punitive states and the ten most punitive states by year. Reading the table from top to bottom should be interpreted as increasing levels of punitiveness among the mass public. One important takeaway from table 3-4 is that with few exceptions, no single state remains in the same rank order—or even in the top or bottom ten states—for very long. This is an important note that reveals the critical variation on both the space and time dimensions.

Table 3-4: Rank Order of States, Least to Most Punitive, by Year

81	82	84	86	87	89	90	91	92	94	95	96	97	98	99	00	01	02	03	04	05	06	07	08	09	10	11	12	13	14	15	16
KS	KS	KS	KS	CA	SD	SD	NM	NM	SD	MI	CT	NY	NY	NY	NY	NY	NY	NY	NY	NY	NY	NY	NY	NY	MA	MA	MA	MA	VT	VT	VT
SC	LA	LA	LA	KS	NV	NV	SD	SD	NM	CT	MI	CT	CT	CT	CT	CT	CT	CT	CT	CT	CT	CT	CT	CT	NY	VT	VT	VT	MA	MA	CT
MA	MA	CA	CA	LA	NM	NM	NV	NV	MI	PA	NY	MI	MI	MI	MI	NH	NH	NH	MA	MA	MA	MA	VT	VT	CT	CT	CT	CT	CT	CT	MA
LA	SC	MA	WY	SD	PA	PA	PA	PA	PA	SD	PA	PA	NM	NM	NH	ME	ME	MA	NH	NH	NH	VT	CA	CA	VT	NY	CA	CA	CA	CA	CA
TN	CA	WY	SD	WY	CA	IL	IL	IL	IL	IL	NM	NM	PA	ME	NM	MI	MA	ME	VT	VT	VT	CA	NJ	NJ	CA	CA	NY	WA	WA	WA	RI
CA	TN	SC	OR	NV	IL	WY	MI	MI	CT	NM	IL	IL	ME	PA	ME	NM	MI	VT	NJ	NJ	NJ	NJ	NH	WA	WA	WA	WA	NY	NJ	RI	WA
VA	VA	OR	NV	PA	WY	MI	WY	HI	NV	NY	SD	ME	IL	NH	PA	RI	NM	NJ	ME	ME	CA	NH	WA	ME	NJ	NJ	NJ	NJ	HI	NJ	NJ
WA	WA	WA	PA	IL	MI	CA	HI	WY	ME	ME	ME	SD	RI	RI	RI	PA	RI	MI	MI	CA	ME	ME	ME	NH	ME	HI	HI	HI	RI	HI	VA
RI	RI	WI	WI	OR	OR	HI	CA	AK	HI	NV	RI	RI	NH	IL	IL	MA	VT	NM	CO	MI	MI	WA	MI	RI	OR	ME	RI	RI	NY	VA	HI
NC	MD	IN	IN	MI	HI	AK	AK	ME	NY	HI	NV	NV	SD	SD	NJ	NJ	NJ	RI	NM	CO	CO	MI	RI	OR	RI	RI	ME	ME	VA	NY	MD
OK	NV	OK	AL	AL	MD	MD	VA	DE	DE	DE	DE	DE	KY	DE	MD	MD	NC	IO	NC	IO	UT	FL	FL	FL	KY	TX	NC	NC	NE	MO	WV
MI	HI	NJ	NC	DE	TN	NJ	VT	VA	AL	VA	VA	VA	DE	TX	VA	VA	NE	NE	NE	NE	NE	UT	KY	KY	LA	MS	TX	NE	MS	NE	AL
NV	IL	NE	DE	NC	NC	NH	AL	AL	VA	AL	MD	MD	TX	KY	NC	NC	TX	TN	TN	TN	FL	NE	AR	TX	TX	NE	NE	MS	WV	WV	NE
NE	OK	VT	AZ	NJ	NJ	VT	MD	AZ	AZ	MD	AL	NC	AZ	AZ	TX	NE	OK	TX	TX	TX	TN	KY	NE	NE	NE	UT	MS	UT	AL	AL	OK
NM	NE	WV	NJ	AZ	VT	TN	AZ	MD	TN	TN	NC	AZ	NC	NC	KY	TX	TN	KY	KY	KY	KY	TX	TX	KS	UT	LA	TN	TN	TN	TN	MO
IL	NM	OH	WV	VT	AZ	NC	TN	TN	MD	AZ	AZ	AL	AL	AL	TN	TN	KY	OK	AR	FL	TX	AR	UT	UT	KS	KS	LA	AL	ND	OK	TN
MN	MN	MT	VT	WV	NH	AZ	NC	NC	NC	NC	TN	TN	TN	TN	AL	AL	AL	AL	FL	AR	AR	TN	TN	TN	TN	TN	KS	WV	UT	UT	KS
MT	MT	FL	FL	FL	WV	WV	WV	WV	WV	WV	FL	FL	FL	FL	FL	FL	AR	AR	AL	AL	AL	AL	AL	AL	AL	AL	AL	KS	OK	KS	LA
OH	OH	AR	NH	NH	FL	FL	FL	FL	FL	FL	WV	WV	AR	AR	AR	AR	FL	FL	OK	OK	OK	OK	OK	OK	WV	WV	WV	OK	KS	ND	UT
NH	NH	NH	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR	WV	WV	WV	WV	WV	WV	WV	WV	WV	WV	WV	WV	OK	OK	OK	LA	LA	LA	ND

Take, for example, the state of Kansas and the commonwealth of Pennsylvania. Kansas, for the first five years of the time-series—1981 through 1987—was either the least punitive state or second least punitive. Then, in the last eight years of the data, Kansas was among the most punitive states in the nation, making an appearance in the top ten most punitive states every year from 2009 through 2016. Over the span of three and a half decades, Kansas went from being home to the least punitive public in the country to home of one of the most punitive.

By contrast, Pennsylvania, from 1986 through 2001, had a remarkable run as one of the least punitive states in the country. For that entire 16-year period, Pennsylvania was in the bottom 10 states in terms of its residents expressing punitive attitudes. Moreover, for much of that span, the commonwealth held remarkably constant at the same rank among the states. In addition to its constant ranking among states, over the course of this period, Pennsylvania's level of punitiveness remained relatively constant.

Why focus on Kansas and Pennsylvania? Two major pieces of existing research on the impact of judicial elections on sentencing have placed their analytical focus on these two states. First, Huber and Gordon (2004) explored the effect of election timing on sentencing decisions by Pennsylvania trial judges from 1990-1999 and reported that electoral considerations account for sentences that are "about three to four-and-one-fourth months longer" (255). In another piece of research, these same analysts found that, between 1997 and 2003, trial judges in Kansas' partisan districts sentenced defendants to terms of incarceration that were "about 2.5 to 3.7 months [longer than a] prison sentence in a retention district," an increase of between 7.8 and 11.6% (Gordon and Huber 2007, 125).

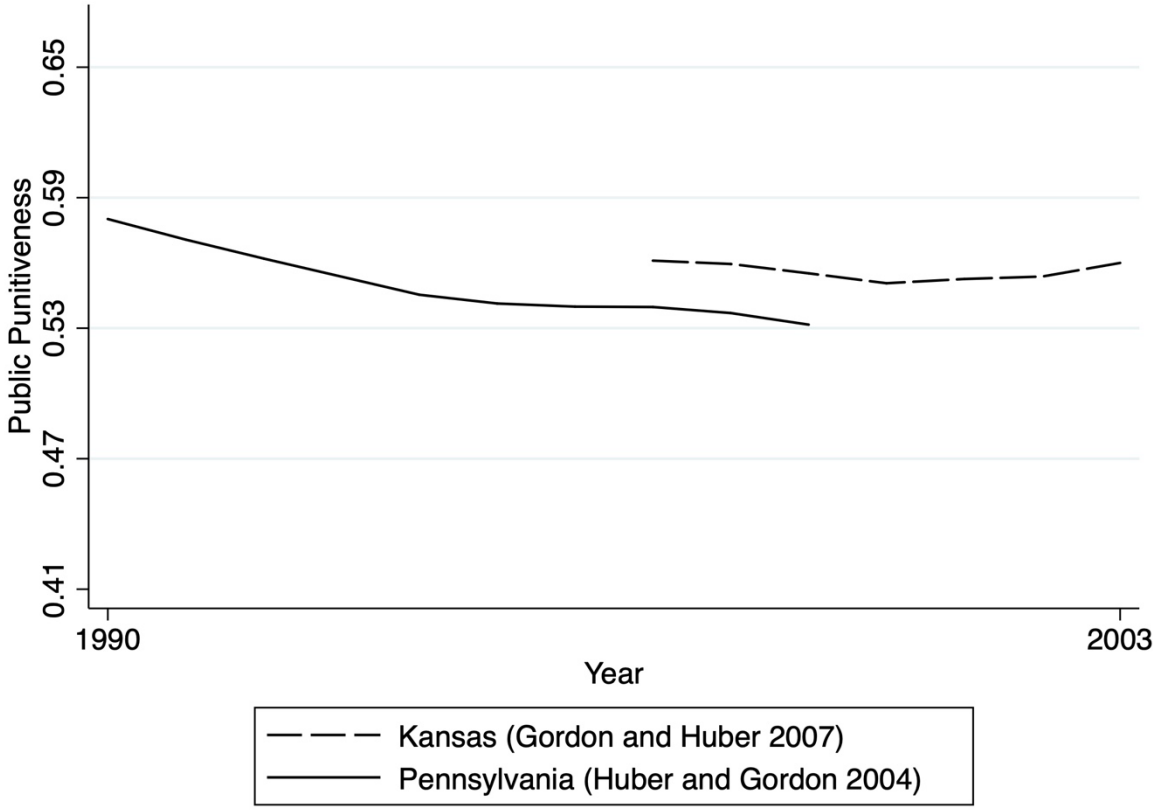
The authors of these studies argue that judicial elections lead to longer sentences. Of course, that argument is predicated on the assumptions that the public indeed prefers punitive responses to criminal behavior and is sufficiently aware of the judges' behavior in response to those preferences. My research rests on the same set of assumptions. However, I take the additional step to prove the punitive public assumption.

The problem with the Gordon and Huber research is that the level of punitiveness in these states would have to be sufficiently variable over the course of their studies. That clearly is not the case, as figure 3-3 demonstrates. The Y-axis in figure 3-3 reports the full range of *Public Punitiveness*, .41 to .67, which is the proportion of the population expressing punitive attitudes in a given state-year. During the period under study in Huber and Gordon (2004), which analyzed sentencing in Pennsylvania, the level of punitiveness fluctuated by only about 5% with very little variation from the mean ($\sigma = .017$). Moreover, the movement present in the trend line for punitiveness in Pennsylvania is negative, indicating a decline in public punitiveness over time. Declining punitiveness as elections approach calls into question the evidence presented by Huber and Gordon (2004).

In Kansas during the period of analysis in Gordon and Huber (2007), there is even less variation ($\sigma = .004$) in the proportion of punitive responses. The time trend is mostly flat over the course of the seven-year period of analysis. If punitiveness is not motivating judges as elections become closer, the question remains unanswered, what is the mechanism by which elections are creating the conditions for increased punitiveness?

In the chapters that follow, I demonstrate that public opinion does play a conditioning role on judicial elections, but it is necessary to conduct an analysis in which variables of interest actually vary. Thus, a cross-state, over-time analysis is warranted.

Figure 3-3: Proportion of Punitive Respondents in Kansas, 1997-2003, and Pennsylvania, 1990-1999



Validity of the PADS

I demonstrate the validity of the MRP estimates by conducting an analysis that closely resembles the state-level analysis conducted by Enns (2016). Formally, I am testing H5, the *Public Opinion Hypothesis*, which predicted a positive relationship between public punitive mood and incarceration rates. Enns demonstrates that increases in public punitiveness at time *t-3* significantly drive incarceration rates upward in the states (see table 6.1, column 3, on page 141), controlling for social and political factors. The middle column in table 3-5 presents the coefficients from Enns' table 6.1. The regression results I

present in the next column of coefficients, labeled Model 3-1, replicates Enns' analysis, with similar results.[11]

The dependent variable in my regression model (3-1) in table 3-5 is the change in state incarceration rates from time *t-1* to time *t*. Incarceration rates are measured per 100,000 population. The regressor of interest is the newly created variable from PADS measuring state punitive mood, *Public Punitiveness*$_{t-3}$, which is the Lowess smoothed measure of punitive attitudes, lagged by three years to be consistent with Enns' analysis.

As in Enns' model, my results indicate that higher public punitive mood is associated with a significant increase in state incarceration rates, controlling for other social and political variables, confirming H5. Holding all other factors constant at their means, a 1% increase in the number of state residents expressing punitive opinions results in an increase in the state's incarceration rate three years later by an average of 77 prisoners per 100,000 population. This shift in incarceration rates that follows changes in *Public Punitiveness* is equivalent to shifting the mean incarceration rate by nearly a half of a standard deviation for every 1% increase in *Public Punitiveness.* To think about this number in more concrete terms by way of example, in the average state-year with a mean incarceration rate of 295.62 inmates per 100,000, a 1% increase in the population expressing punitive attitudes would increase the incarceration rate by more than one-quarter. The public's mood is clearly impacting incarceration rates, and the effect of

[11] My analysis differs from Enns' in two technical ways. First, although we both use linear regression, he uses bootstrap cluster standard errors, while I opt for panel-corrected standard errors to account for the panel nature of the data. Second, Enns uses two variables to measure political control in the state—an indicator variable for a Republican governor and the percentage of Republicans in the state legislature—while I use a single count variable that measures Republican control of state government.

punitive attitudes is far-and-away the single strongest predictor of incarceration rates in the model.

Table 3-5: Relationship between Public Punitiveness and State Incarceration

	Enns, Column 3	Model 3-1
Public Punitiveness$_{t-3}$	0.24*	77.090*
	(0.06)	(36.480)
Δ Incarceration Rate$_{t-1}$	0.125	–0.003
	(0.081)	(0.015)
Violent Crime Rate $_{t-1}$	0.013*	0.007*
	(0.002)	(0.003)
State GDP $_{t-1}$	–0.06	28.782*
	(0.05)	(10.417)
Top 1% Income Share $_{t-1}$	3.29	0.115
	(17.01)	(0.272)
% African American $_{t-1}$	–0.06	0.508*
	(0.09)	(0.124)
Republican Control $_{t-1}$	—	–2.726*
		(0.563)
Republican Governor $_{t-1}$	–0.29	—
	(0.70)	
% Republican in Legislature $_{t-1}$	3.99	—
	(2.75)	
Northeast	–2.66	8.567*
	(1.40)	(2.910)
Midwest	–1.48	13.294*
	(1.31)	(2.832)
West	–2.25	11.772*
	(1.62)	(2.617)
Constant	–11.82*	–67.405*
	(4.15)	(23.900)
R^2	0.08	0.014
No. of States	49	49

Note: The dependent variable is the change in state incarceration rates. The middle column reports OLS coefficients with bootstrap cluster standard errors in parentheses for years 1953–2010, which are reported by Enns (2016, 141) in table 6.1, Column 3. The Enns model reports an Adj. R^2. Model 3-1 reports linear regression coefficients with panel corrected standard errors in parentheses. * = $p<0.05$; two-tailed test. Only 49 states are included because Nebraska has a non-partisan legislature. Results from different length lags of *Public Punitiveness* are reported in table A3-4 in the appendix and show that results are sensitive to the lag length.

I also control for the violent crime rate, the state's GDP as a measure of the state's economic health (Rosenfeld 2009; Rosenfeld and Fornango 2007), the proportion of income received by the top 1% of each state as a measure of inequality (Frank 2009) to

account for the social control hypothesis (Chambliss and Seidman 1971), the percent of the state population that is Black (Alexander 2010), and Republican control of the state with a count variable of the number of branches of government controlled by Republicans (Petrocik et al. 2003). Each of these control variables is lagged by one year. I also include indicator variables for each region of the country with the South as the excluded category.

Consistent with Enns' results, I find that the violent crime rate increases incarceration rates. Enns finds that the percent of the state that is Black has a null effect on the incarceration rate, but I find that—consistent with the racial threat hypothesis—as the percentage of a state's population that is Black increases by 1%, the incarceration rate rises by about one inmate per 200,000 population. Although many scholars are critical of the racial threat hypothesis (see, e.g., Acharya et al. 2016; Voss 1996)—that a White majority is threatened by the presence of large proportions of racial minorities and seek to suppress their power through the power of the state—it appears that, at least in the data I use here, the hypothesis holds some sway.

The coefficient for state GDP is significant; for every increase of $100,000 in state gross domestic product, incarceration rates rise the following year by nearly 29 inmates. Enns reports no effect on incarceration of party control of the government in his main model, which I present in table 3-5 for comparison, I find that states that are controlled by Republicans have lower incarceration rates than states where Democrats control state government.[12] This finding is contrary to previous scholarship (e.g., Yates and Fording

[12] Although his main analysis, which covers the years 1953–2010, reports no partisan effects, Enns' supplemental analysis, which is limited to 1970–2000, finds a significant effect of partisanship on incarceration. Specifically, he reports that a greater share of Republican legislators increases incarceration, a finding that is significant at the 0.10 level (Enns 2016, 142, 150–1).

2005) and is worthy of future exploration, but is beyond the scope of the current work. Finally, the South has statistically lower rates of incarceration than any of the other three regions of the country, a finding that is consistent with previous scholarship (e.g., Yates 1997), but that differs from Enns' analysis.

Demonstrating that punitiveness exerts such a strong and significant impact on incarceration rates serves to validate the measures of punitiveness contained in PADS. Had my measure of punitiveness failed to replicate the substance of Enns' findings, the PADS measures could have rightly been called into question. This is, however, fortunately not the case. The results presented here should give the reader confidence not only that the PADS is actually measuring punitiveness but also that it is measuring the concept correctly and validly. Data from PADS are used in the two subsequent chapters of this dissertation to test hypotheses related to the interaction between judicial retention mechanisms and public opinion on crime and punishment.

Future Research Using PADS

Scholars from across a host of disciplines could make use of PADS. Research programs from political science to criminology to public health stand to benefit from the data. Indeed, scholars from multiple disciplines have already begun the task of evaluating public opinion in the criminal justice environment. Scholars have investigated the antecedents of punitive attitudes (Gromet and Darley 2011; Ramirez 2013); racial differences in attitudes toward the justice system (Gibson and Nelson 2018; Hurwitz et al. 2015; Peffley and Hurwitz 2010; Peffley et al. 2017); and the implication for such opinions for public policy outcomes, such as prosecutorial and sentencing behavior (Nelson 2014), incarceration rates (Enns 2014, 2016; Jennings et al. 2017), prison privatization (Enns and

Ramirez 2018; Frost et al. 2019), and the use of capital punishment (Brace and Boyea 2008; Canes-Wrone et al. 2014; Lax and Phillips 2012). However, as discussed previously, many of these studies use public opinion data that lack variation on one of the two important dimensions, if they use public opinion data at all.

PADS provides data that can bolster the analyses conducted at the state level. Beyond the hypotheses tested in this dissertation, research using PADS could analyze whether levels of prison privatization are thermostatically responsive over time to public opinion at the state level, or whether state legislators and governors respond to changes in punitive mood with the types of legislation passed. Other research could investigate the political (rather than policy) implications of dynamism in punitive mood. For example, does a rise (or fall) in public punitiveness at the state level lead to an increased vote share for the Republican (Democratic) candidate for governor, or does it shape the state's preference in a presidential election? With temporally and spatially dynamic data, such as that contained in PADS, questions like these become more readily answerable by scholars interested in the public opinion component of criminal justice and the punitive mood aspect of political behavior.

Conclusion

In this chapter, I have introduced a new dataset that contains measure of punitive mood in the states that varies both across states and over time. In introducing the PADS, I both overcome previous problems in the literature and provide the data necessary to test the dynamic responsiveness of state criminal justice policy to public mood. Previous research exploring punitive attitudes have lacked variation on one of two critical dimensions. Measures of punitiveness that varied by state were temporally static, i.e., they

did not vary over time, while research measuring punitiveness over time did so at the national level and did not permit punitiveness to vary from one state to another. Data in PADS varies on both of these critical dimensions.

Data from PADS are used in the two subsequent chapters of this dissertation to test hypotheses related to the interaction between judicial retention mechanisms and public opinion on crime and punishment. In chapter 4, a three-year lagged measure of punitive mood, $Public\ Punitiveness_{t-3}$, is incorporated in several statistical models as both a variable of interest as a constituent term and as a multiplicative term when investigating how judicial elections and public opinion interact to produce variable levels of sentencing to the carceral system and of incarceration population rates. In chapter 5, the same measure is used in models testing the effect of public punitive moods and the mood-elections interaction on racial disparities in state prison systems.

Chapter 4: Elected Punitiveness: Judicial Elections, Public Opinion & Incarceration Rates

Chapter Overview

In chapter 2, I developed a theory of democratic responsiveness of elected judges. I posited that elected judges will be responsive to public opinion and that at the state level, that behavior would be manifest in higher levels of punitive criminal justice outcomes, especially in states where punitive mood was higher. In chapter 3, I introduced a state-level dataset of punitive attitudes among state publics that I contend will condition the impact of judicial retention methods on incarceration rates. In this chapter, I test hypotheses using the change in a state's *Incarceration Rate* in a given year from the prior year as the dependent variable.

I begin this chapter with an introduction of the mass incarceration problem in the United States and the political antecedents of mass incarceration. I then review the relevant theory, data, variables, and methods I use to investigate the relationship between judicial retention institutions and state incarceration rates. After presenting the results of both time-series and causal analyses I discuss the meaning of these findings. I conclude with a discussion of important remaining questions that deserve a closer look in future research.

Introduction

The United States incarcerates more of its population than any other nation in the world. Scholars across the social sciences, especially criminologists (Costelloe et al. 2009), sociologists (Johnson 2001; Schoenfeld 2018), and economists (Lim 2013; Park 2017) have investigated the precursors of the growing incarceration rates in the United States. Political scientists (Gordon and Huber 2007; Huber and Gordon 2004; Moore 2015; Yates and Fording 2005) and other social scientists (e.g., Johnson 2001; Lim 2013; Park 2017;

Schoenfeld 2018) have devoted particular attention to the role politics plays in the criminal justice system and to the political antecedents of high incarceration rates (Enns 2014, 2016; Schoenfeld 2018). However, no systematic, comparative work has been done to investigate the effect of judicial elections on state-level incarceration rates. Moreover, much of the research on the policy implications of judicial elections focuses on state appellate courts, especially state supreme courts (Blake 2018; Brace and Boyea 2008; Brace and Hall 1995; Canes-Wrone et al. 2014; Hall 2001). Relatively few studies have endeavored to study state trial courts (for exceptions, see Gordon and Huber 2007; Huber and Gordon 2004; Lim 2013; Nelson 2014; Park 2017; Rachlinski et al. 2009), and the ones that do are often limited geographically, leaving a gap in the literature that raises an important question: does the retention mechanism for trial judges influence a state's incarceration rates? I fill that gap with the first cross-state analysis of trial judge elections and criminal justice outcomes that encompasses all 46 states that have a uniform mechanism for trial judge retention.

A long line of research on judicial behavior demonstrates that judges are not merely umpires calling balls and strikes, as Chief Justice John Roberts famously quipped during his confirmation hearing. Judges are motivated by myriad extra-legal considerations including their ideology (Segal and Spaeth 2002), strategic considerations (Epstein and Knight 1998), prestige for their institution (Hinkle and Nelson 2016), lived experiences (Haire and Moyer 2015), or how recently they have eaten (Danziger et al. 2011). Similar to their legislative contemporaries, judges beholden to voters are likely, to some extent, motivated by the desire for re-election (Mayhew 1974).

Previous research demonstrates the importance of electoral considerations for judges by analyzing policy outcomes that are (1) directly tied to the behavior of judges and (2) sufficiently salient to voters to provide electoral motivation (Brace and Boyea 2008; Brace and Hall 1995; Caldarone et al. 2009; Canes-Wrone et al. 2014; Canes-Wrone et al. 2012; Lim 2013; Nelson 2014; Park 2017). Incarceration rates meet both these criteria. Research shows that incarceration rates are correlated with the punitive attitudes of the public (Enns 2014, 2016; Jennings et al. 2017) and provide trial judges the opportunity to curry favor with a punitive public on a highly salient issue—i.e., crime (Levitt 1997; Peffley and Hurwitz 2010; Shugerman 2012).

Based on this argument, I hypothesize that incarceration will be higher in states with judicial elections than in states without them and that the public's punitive mood will condition those institutional arrangements. To test my hypotheses, I first create state-level measures of year-to-year changes in incarceration rate changes between 1978 and 2015 in all 50 states based on a dataset of state prison populations (NPS). I then estimate a sequence of pooled time-series regression equations to determine whether different types of judicial election mechanisms influence punitive policy outcomes in the states. In the time-series analysis, I use panel-corrected standard errors (PCSE) to allow for better inference from linear models estimated from time-series cross-sectional (TSCS) data (Beck and Katz 1995).

The results support the hypothesis that judicial elections are associated often with greater punitiveness in the states, conditioned by public opinion. Two primary findings are critical to note. First, states where judges remain on the bench via popular election, regardless of the electoral system used, are more responsive to public opinion than non-

electoral states, and the differences in the penal population is often significantly higher in the electoral states where the public exhibits a more punitive mood. Second, comparing election types to one another, non-partisan elections exhibit greater plebiscitary pressure than states with partisan elections. As the public's mood becomes more punitive, states with non-partisan elections exhibit greater increases in incarceration rates, compared to states with partisan elections.

An Empirical Test

Recall from chapter 2 that the Theory of Elected Punitiveness contends that (1) crime is a salient issue in American politics; (2) the public, on average, prefers punitive, rather than rehabilitative, responses to crime; (3) incarceration rates are thermostatically responsive to punitiveness in public opinion; (4) judges who seek their seat on the bench via elections must be responsive to public opinion because (5) they are subject to the same electoral considerations as other politicians, namely the desire to win re-election. Thus, the theory predicts that in jurisdictions where judges are re-elected, the levels of punitiveness should be higher than in non-electoral jurisdictions. Formally, I test the *Elections Incarceration Hypothesis (H1)*:

> States with judicial elections to retain trial court judges will have higher annual changes in incarceration rates than states without judicial re-election.

Furthermore, recall, however, that not all judicial elections are alike. This analysis distinguishes between the three types of judicial elections in the states: uncontested retention, non-partisan, and partisan elections. I theorized that the party label of judicial candidates who almost always run in low-information races is the most important heuristic for American voters. When that heuristic is present in judicial races, some judges may be

able to fly under the radar by being less punitive but relying on a partisan cue to persuade voters that they are tough on crime. However, when institutional arrangements prohibit that heuristic, as in the case of non-partisan elections, judicial incumbents should be even more motivated to (1) behave on the bench in a way that signals to voters they are responsive to their concerns about crime and public safety (2) by accumulating a track record as a trial judge that provides them with a ready-to-run campaign message that they are tough on crime (3) that gives voters a retrospective accounting of the judge's effectiveness on the bench and a reason to vote to return the judge to office. Thus, I test the *Non-Partisan Elections Punitive Hypothesis (H2)*:

> States with judges re-elected in non-partisan elections will have higher annual changes in incarceration rates than states with partisan judicial elections.

I use the change in a state's incarceration rate from year-to-year as reported in the NPS dataset (Bureau of Justice Statistics 2017) to test these hypotheses. The dependent variable is the state's *Incarceration Rate*, which is the number of inmates incarcerated per 100,000 population in a state in a given year, as it changed from the previous year. The national aggregate measure of *Incarceration Rate* from 1978-2015 is reported in figure 4-1, while figure 4-2 displays the yearly trend in incarceration rates for each state over the same period. The annual change in *Incarceration Rate* is a normally-distributed, continuous variable.[1] Incarceration rates have been found to be responsive to changes in the punitive attitudes of the public (Enns 2014, 2016; Jennings et al. 2017), making a state's

[1] Histogram plot for the annual change in *Incarceration Rate* is located in the appendix, figure A2-1.

incarceration rate an important indicator for understanding the judicial system's responsiveness to public opinion.

Figure 4-1: Annual Change in the National Incarceration Rate per 100,000

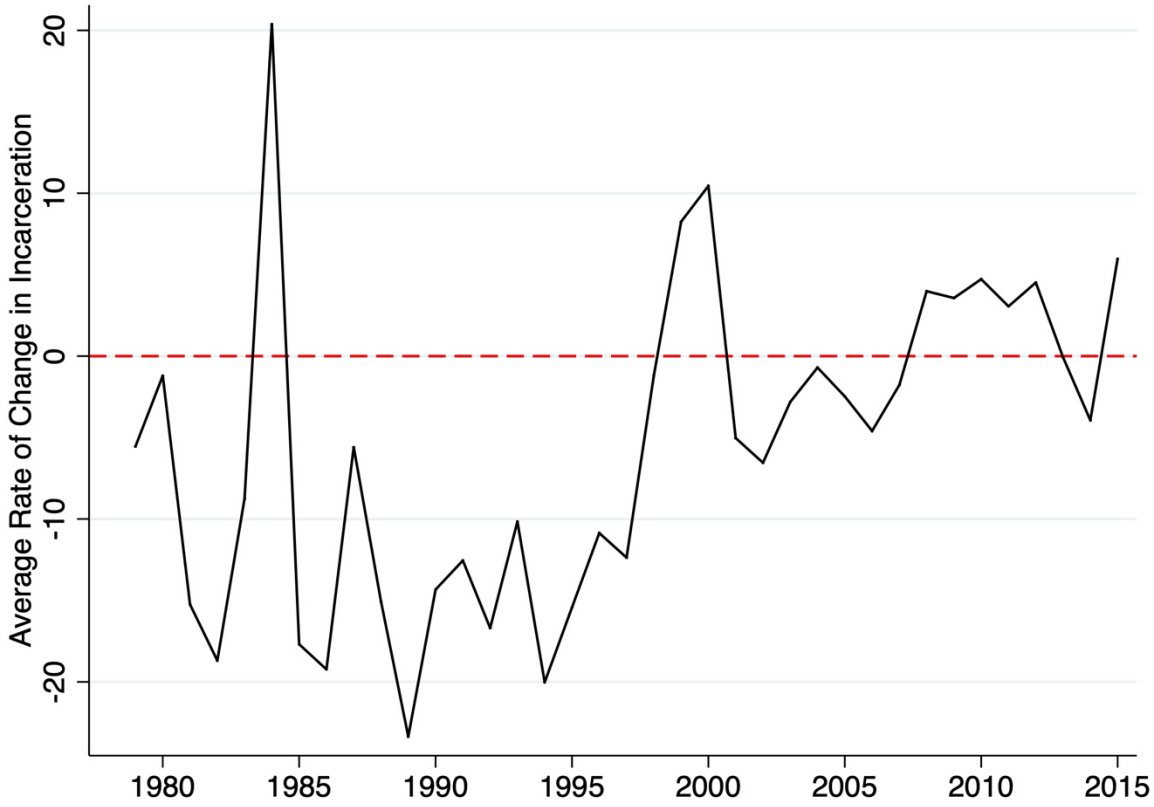


Figure 4-2: Annual Trends in Incarceration Rates, by State

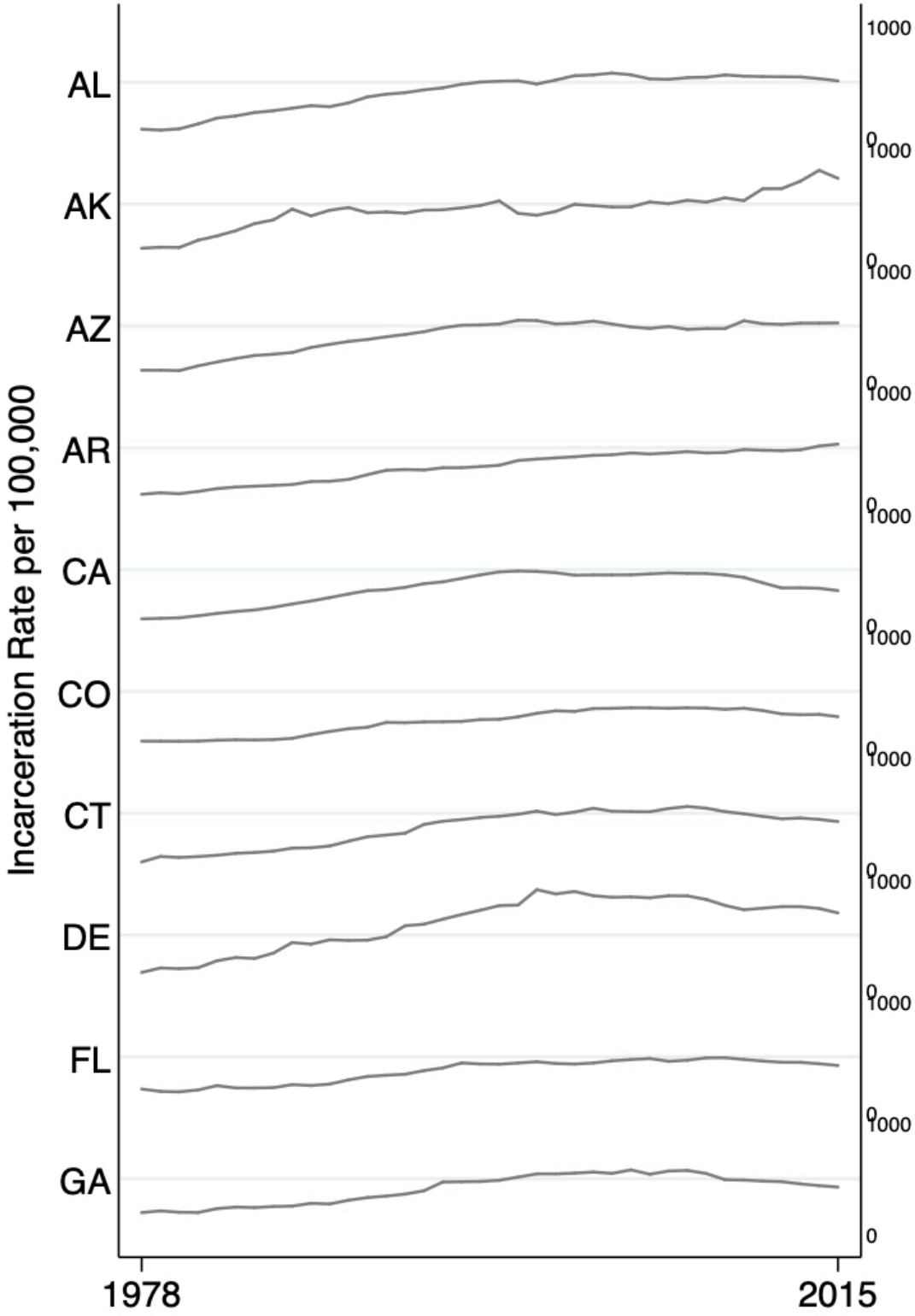


Figure 4-2: Annual Trends in Incarceration Rates, by State, cont.

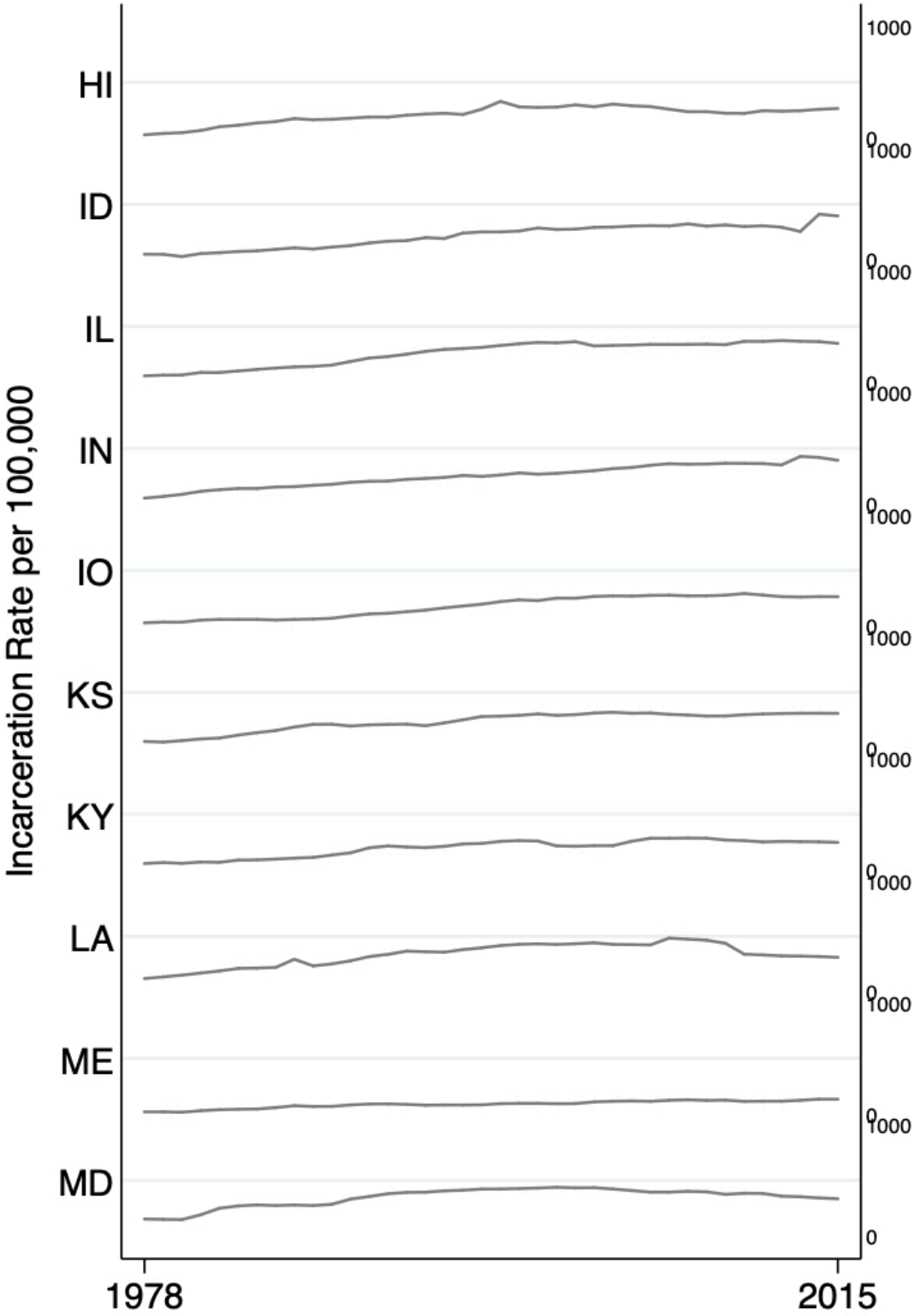


Figure 4-2: Annual Trends in Incarceration Rates, by State, cont.

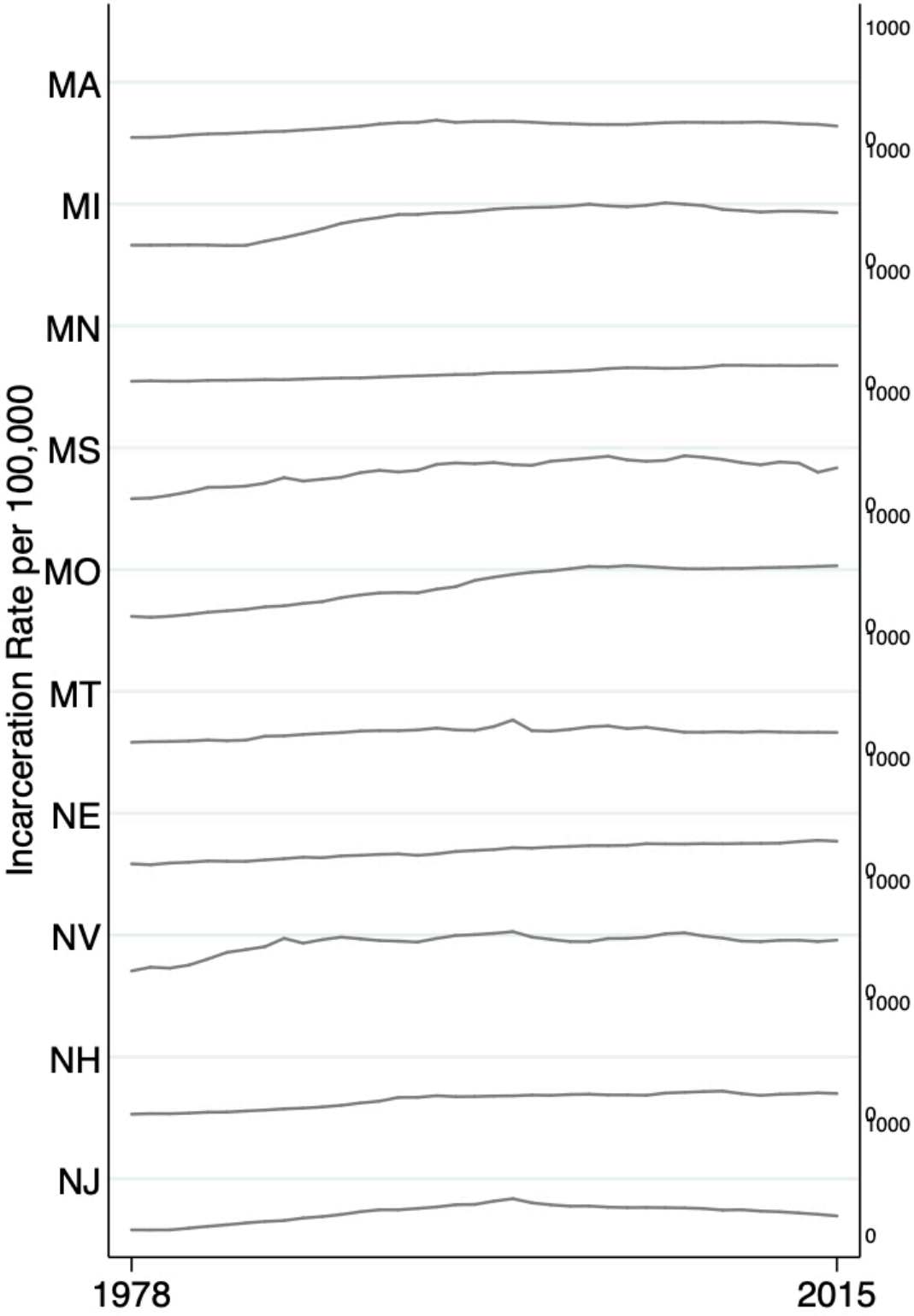


Figure 4-2: Annual Trends in Incarceration Rates, by State, cont.

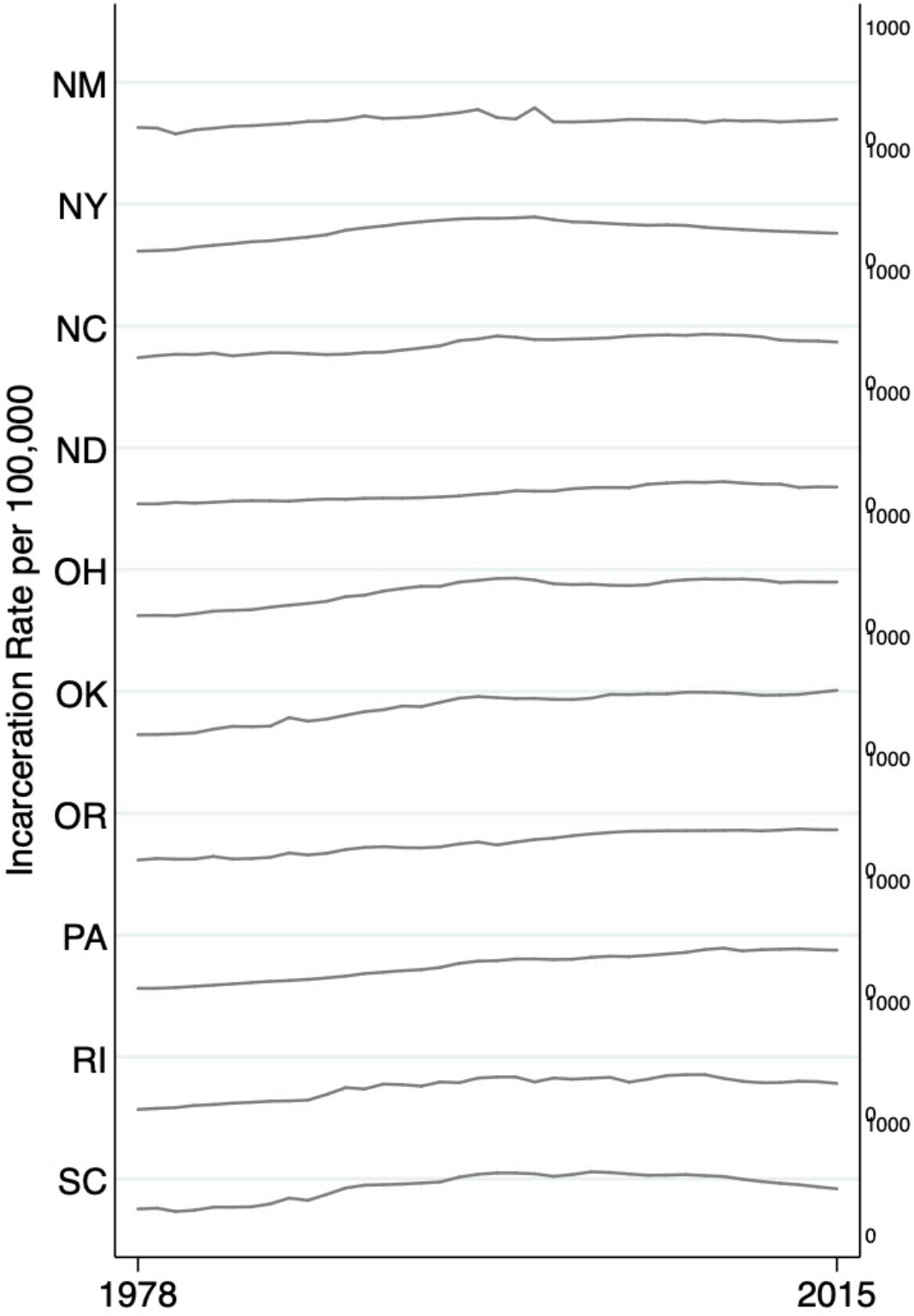
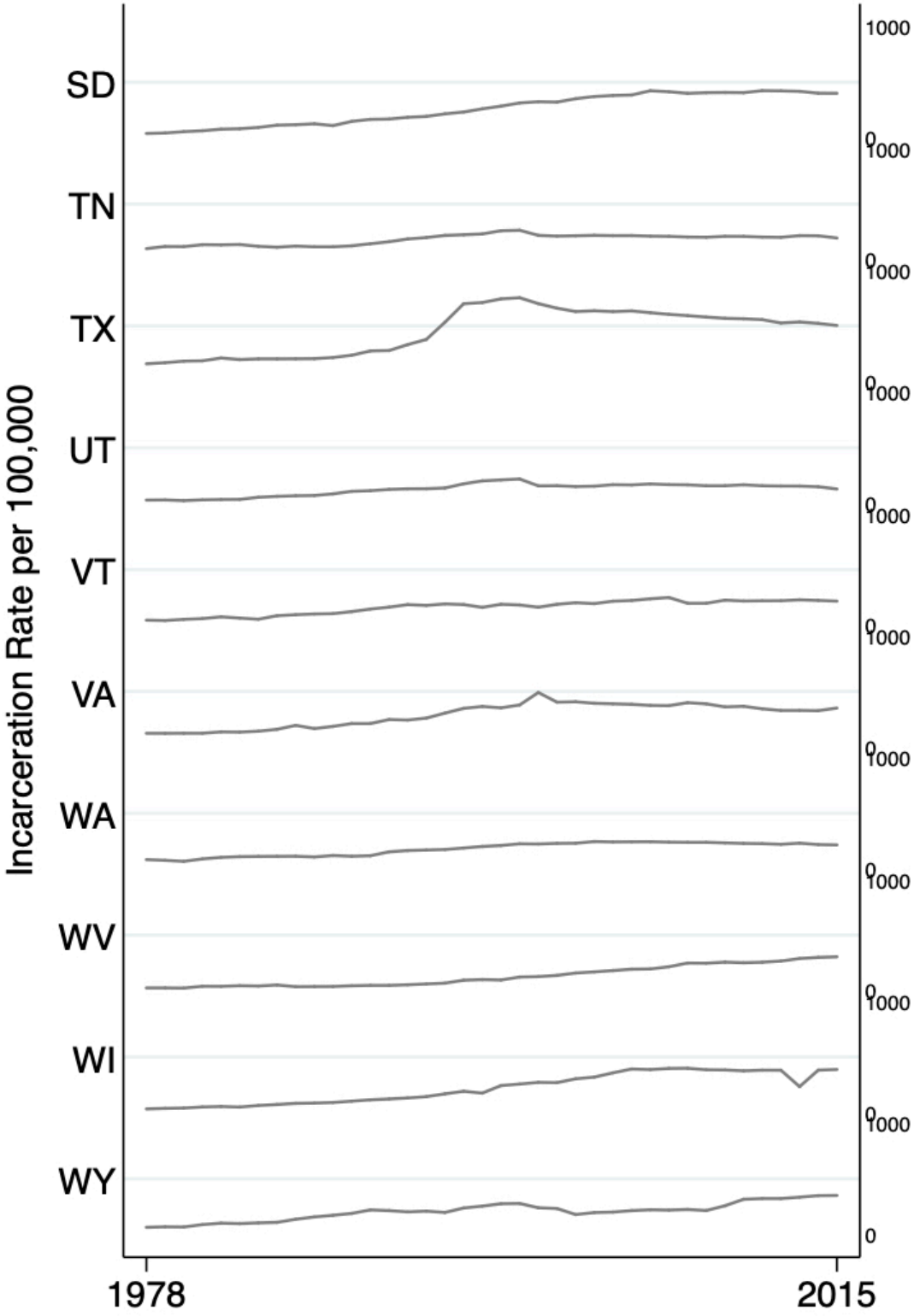


Figure 4-2: Annual Trends in Incarceration Rates, by State, cont.



States are displayed in alphabetical order. The horizontal axis represents the year (1978-2015) and the vertical axis is the state's incarceration rate per 100,000 population; all vertical axes are scaled from 0-1000 for direct comparability.

A series of three indicator variables, collectively labeled *Retention Mechanism*, capture the different types of state judicial retention procedures, with states where judges do not face election to retain their seat on the bench serve as the reference category (coded 0). States with *Uncontested Retention Elections*, *Non-Partisan Elections*, and *Partisan Elections* are coded 1 for the three indicator variables.[2]

In addition to the mechanism of judicial retention, I am also interested in the impact of public punitiveness on punitive outcomes. To account for public punitiveness, I include the variable, *Public Punitiveness*$_{t-3}$, from the PADS dataset introduced in chapter 3, where state punitiveness, based on the MRP estimates, is lagged three years. I also include a multiplicative term between each of the three retention mechanism indicators and *Public Punitiveness*$_{t-3}$ to test the hypotheses that states with elected judges are more responsive to the public's punitive mood than are judges in states without judicial elections.

I also include controls for a number of factors that either have been demonstrated to, or potentially could, *a priori*, influence incarceration rates. Recall from chapter 2 that there are four types of control variables: political, legal, demographic and economic factors, as depicted in table 4-1.

Table 4-1: Control Variables

Political Factors	Legal Factors	Geographic-Demographic Factors	Economic Factors
Elected Prosecutors	Arrest Rate	South	Income$_{t-1}$
Judicial Term Length	No IAC	% Black	
Recall		% Black^2	
		% Bachelor's Degrees	

[2] See figure 1-1 in chapter 1 and tables 2-2 and 2-3 in chapter 2.

The annual change in *Incarceration Rate* is normally-distributed and continuous, so I estimate the relationship between the retention methods and *Incarceration Rate* using linear regression. However, OLS is not appropriate for time-series cross-sectional (TSCS) data due to concerns about autocorrelation and heteroskedasticity. To correct for these problems arising from the use of TSCS data, I employ panel-corrected standard errors (PCSE) (Beck and Katz 1995) and panel-specific autocorrelation corrections (Beck and Katz 2007). The unit of analysis in each model is the state-year.

I begin by reporting the findings of a baseline model that estimates only the effects of the judicial retention indicators and the measure of public punitiveness on the change in states' incarceration rates. The coefficients for the three indicator variables capture the unconditional difference between the three types of judicial elections (i.e., *Uncontested Retention Elections*, *Non-Partisan Elections*, and *Partisan Elections*) versus states where judges face no election. As reported in model 4-1 in table 4-2, the baseline estimates for the retention variables public punitiveness do not support the hypothesis that either judicial elections or public opinion are associated with a change in incarceration rates in the states. Of the variables in this model, only the coefficient for *Partisan Elections* is statistically significant, but it is negative, indicating that states with partisan elections have smaller annual changes in incarceration rates than states without judicial elections.

Of course, changes in state incarceration rates are hypothesized to be influenced by more than just retention mechanisms and public punitiveness. Accordingly, model 4-2 adds ten control variables for political, legal, and socioeconomic factors. In this more fully specified additive model, two of the three electoral retention mechanisms are significantly and positively related to the change in *Incarceration Rate*, while *Public Punitiveness*$_{t-3}$ is

positively related to the dependent variable but not significantly so. The *Elections Incarceration Hypothesis* predicts that states with elections to retain trial judges should have higher incarceration rate changes than states without elections. Consistent with expectations, states with *Non-Partisan Elections* and *Partisan Elections* have significantly higher annual changes in state prison populations than states without elections. States that use *Non-Partisan Elections* increase their prison populations by nearly 11 inmates per year compared with states without judicial elections. Prisons in states that use *Partisan* judicial elections swell in size by more than 11 inmates per year compared to states without judicial elections. The coefficient for *Uncontested Retention Elections* is not statistically significant, indicating there is no difference between these states and states without elections. Again, while the effect of state public punitiveness is not statistically significant, it is important to point out that model 4-2 captures the additive effects of both retention mechanisms and public punitiveness.

Before turning to the results of the interactive model, I briefly describe the effects of the control variables. Of the demographic characteristics of the states, only the *Percent Black* populations has a significant impact on changing incarceration rates. As expected, we observe a U-shaped curvilinear relationship as indicated by the negative and significant coefficient for *Percent Black* and the positive and significant coefficient for *Percent Black*^{2}.

Meanwhile the coefficient for South is statistically significant, and the direction of the coefficient sign indicates that southern states have a lower increase in incarceration than non-southern states.[3] As expected, as the state's *Arrest Rate* in the previous year

[3] This result is consistent with what I uncovered in chapter 3 and is equally puzzling here. Future work should explore why the south's incarceration rates are significantly lower than those in the rest of the country.

increases, so do state incarceration rates. Also, states that elect their prosecutors have significantly higher increases in incarceration than states without prosecutorial elections.

On the other hand, judging from the coefficient for *No IAC,* the lowered threat of review and reversal in states without an intermediate appellate court, compared to those with an IAC, has no significant impact on incarceration rates. Other state judicial structures appear to significantly shape incarceration rates, but not in the anticipated direction. Specifically, independent of retention mechanisms and punitiveness, as well as other predictors, longer judicial term length is significantly associated with increasing incarceration rates, while *Judicial Recall* is associated with a decrease in state incarceration rates, indicating that states where judges are subject to recall elections have smaller changes in incarceration than states where judges are insulated from mid-term removal by the voters.

The Conditional Effects of Elections and Punitive Mood

Model 4-3 reports the estimates for a fully specified model including the hypothesized interaction between *Retention Method* and *Public Punitiveness*$_{t-3}$ to test whether public opinion is more important for states with judicial elections as predicted by H6 and H7. Recognizing that the impact of variables can be difficult to interpret in the face of interactions effects (Brambor et al. 2006), to better understand how the impact of judicial elections is conditioned by public opinion, I use the coefficients in model 4-3 to compute the average marginal effects of the difference in the effects of each of the three election mechanisms versus states with no elections on change in incarceration rates across the range of public punitiveness in the states. Thus, figure 4-3, displays the marginal contrasts of predicted incarceration rates for different retention mechanisms (displayed on the Y-

axis) across the range of state public punitiveness (on the X-axis), holding other predictors at their means.[4] The X-axis in the graphs displays the actual range of *Public Punitiveness*$_{t-3}$ in the data, which runs from 0.49 to 0.64.[5]

[4] Tables with margin and contrast statistics are located in the appendix (c.f. tables A4-1 and A4-2).
[5] See table 2-3 in chapter 2.

Table 4-2: Antecedents of State Incarceration Rates

	Model 4-1	Model 4-2	Model 4-3
Uncontested Retention Elections (URE)	–1.44 (2.182)	–7.75 (5.112)	–152.69** (63.745)
Non-Partisan Elections (NPE)	1.89 (1.85)	10.94* (5.940)	–270.43*** (78.376)
Partisan Elections (PE)	–4.47** (2.140)	11.13** (5.622)	–138.22** (57.963)
Public Punitiveness$_{t-3}$	2.89 (35.454)	85.11 (65.423)	–161.51** (63.838)
URE x Public Punitiveness$_{t-3}$	—	—	252.76** (109.250)
NPE x Public Punitiveness$_{t-3}$	—	—	480.66*** (131.919)
PE x Public Punitiveness$_{t-3}$	—	—	254.49*** (97.643)
Arrest Rate$_{t-1}$	—	0.02*** (0.003)	0.02*** (0.003)
No Intermediate Appellate Court	—	–7.18 (4.621)	–7.48 (4.775)
Elected Prosecutors	—	23.30*** (7.795)	24.31*** (7.543)
Judicial Term Length	—	0.21*** (0.064)	0.20*** (0.062)
Judicial Recall	—	–37.95*** (8.407)	–35.34*** (8.771)
South	—	–30.18** (12.663)	–31.82** (12.664)
Percent Black	—	–11.81*** (2.978)	–12.09*** (3.166)
Percent Black^2	—	4.05*** (1.152)	4.56*** (1.171)
Bachelors' Degrees	—	3.57 (6.990)	1.43 (6.600)
State Income$_{t-1}$	—	0.00 (0.001)	0.00 (0.001)
Constant	–4.98 (20.384)	–176.79*** (53.054)	–31.22 (49.802)
N	1,286	1,253	1,253
R^2	0.002	0.336	0.345
Number of States	46	46	46

All models estimated using Stata 15.0's xtpcse command. Data from National Prisoner Statistics, 1978-2015, US Department of Justice, ICPSR Study 36657. DV is the yearly change in a state's incarceration rate per 100,000 population. Correlated panel-corrected standard errors in parentheses. Results of two-tailed tests of significance are as follows: *p< .1 **p< .05 ***p< .01. Autocorrelation corrected using panel-specific AR(1). Unit of analysis is the state-year.

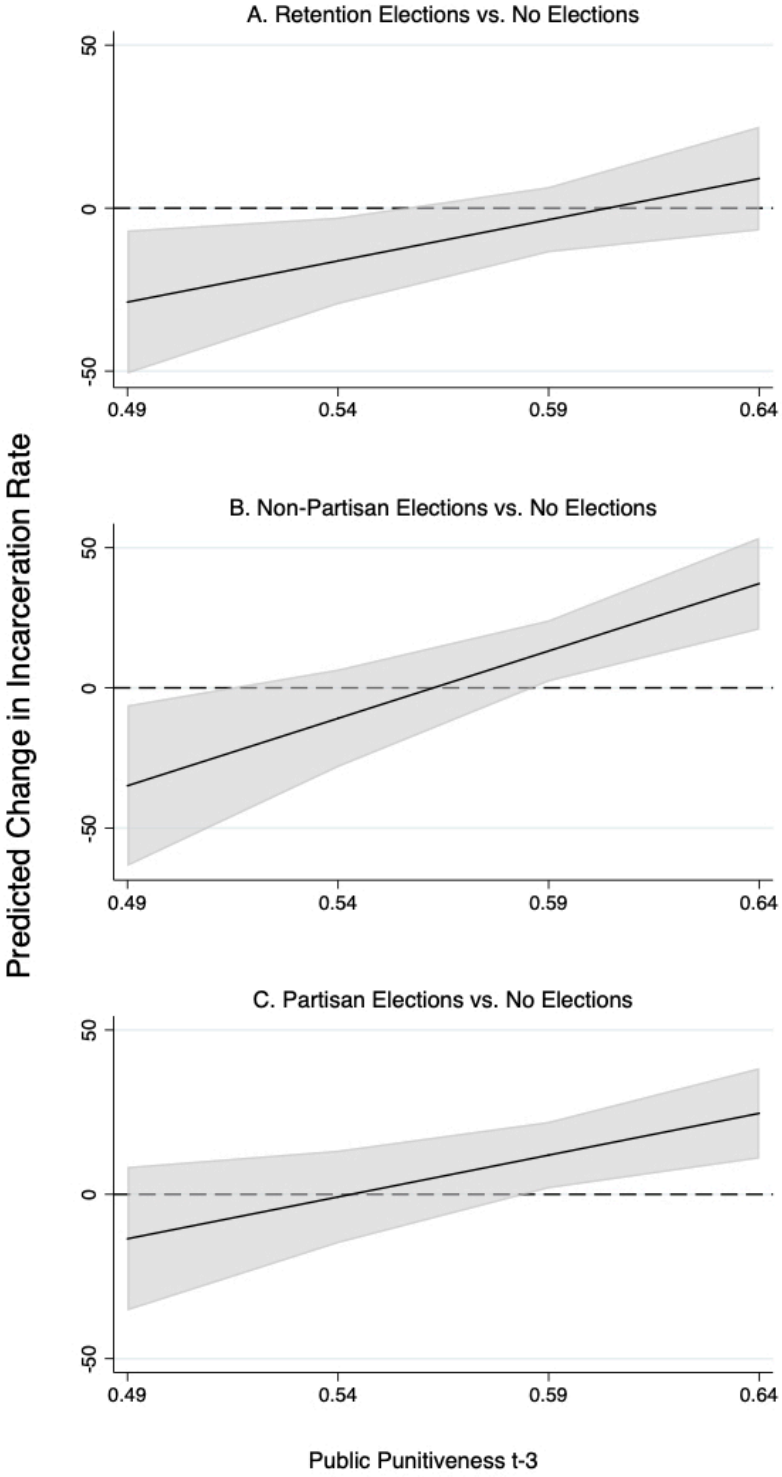
Turning to figure 4-3, which reports the contrasts for *Incarceration Rate*, we see that the effect of judicial elections on a state's changing prison population is conditional on the punitive mood of the public, consistent with the *Public Punitiveness Interaction Incarceration Hypothesis (H6)*. In each of the panels, if elected judges are responsive to public opinion, we should observe a positively sloped line, which indicates that as *Public Punitiveness*$_{t-3}$ increases, so do state *Incarceration Rates*. If the 95% confidence intervals of the slope touch the reference line at Y=0, it indicates that the contrast is not significantly different for that value of punitiveness.

Panel A compares states with uncontested retention elections to states without elections. At lower levels of state public punitiveness, states that select judges with uncontested retention elections lead to a decrease in incarceration rates, compared with states without elections. At higher levels of state punitiveness, however, the contrast in incarceration rates between the two types of selection mechanisms becomes not statistically significant.

Panels B and C of figure 4-3 tell a somewhat similar story. In Panel B, which contrasts the effects of non-partisan elections with no elections on changing state incarceration rates, the slope of the line remains positive, but the differences are statistically significant at different points along the punitiveness scale—namely at the lowest level of punitiveness and for states with punitiveness levels higher than .58. In Panel C, when comparing the contrast in incarceration changes across states with partisan elections versus no elections, once again, the differences become statistically significant only at higher levels of punitiveness, greater than .58. Thus, like non-partisan elections, partisan elections

increase a state's responsiveness to public opinion, even though the contrast is not as great as it is for non-partisan elections (cf., Panel B).

Figure 4-3: Marginal Effects of Electoral Systems, Contrasted with Non-Electoral Systems, Conditional on Public Punitiveness



Note: Contrasts in fitted values on the Y-axis are based on the estimated coefficients in table 4-2. 95% confidence intervals. Contrast statistics and levels of significance are listed in the appendix, table A4-2. NE=307 State-Years (SY); URE=218 SY; NPE=531 SY; PE=230 SY

In short, when compared to states without elections, the impact of judicial retention elections on changing incarceration rates in the states is conditioned by the level of public punitiveness in the states. Overall, incarceration rates are shaped by public opinion, especially in states where judges face the public in an election. Concerning the control variables, the most important observation to make is that the model performs almost identically to the additive model reported in model 4-2. Generally speaking, the findings for the variables of interest are consistent with my expectations. Specifically, states with elections are more responsive to public opinion than states without elections. As *Public Punitiveness*$_{t-3}$ increases in the states, those with elections increase their *Incarceration Rates* more than other states at similar levels of punitiveness but without judicial elections. We can also understand why the level of punitiveness in the baseline and additive models is insignificant, since these models average over different electoral arrangements in the states.

Non-Partisan versus Partisan Elections

One remaining question is whether the effects of non-partisan elections on incarceration rates are greater than those for partisan elections, which prior research suggests is true for state Supreme Courts (e.g., Canes-Wrone et al. 2014). Accordingly, in this section, I evaluate the difference in changing incarceration rates across the two selection mechanisms to test H2, the *Non-Partisan Elections Incarceration Hypothesis*:

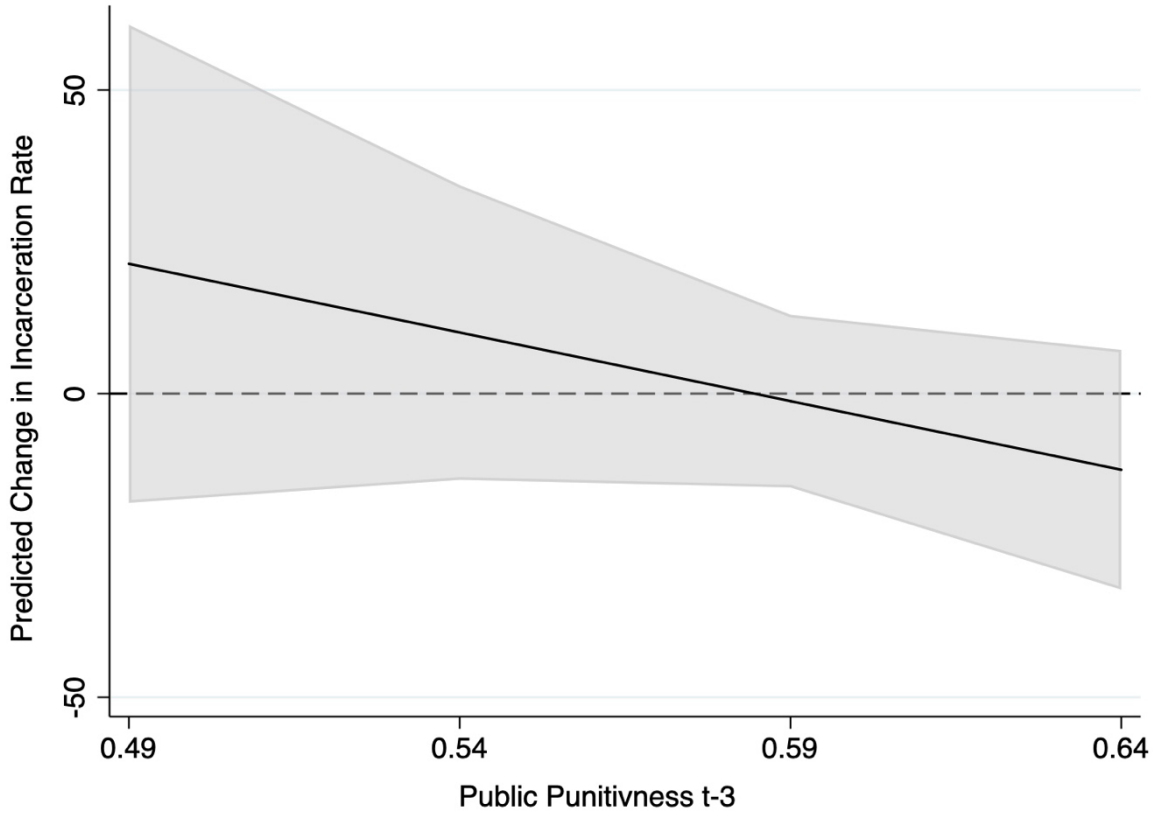
> States with judges re-elected in non-partisan elections will have greater annual changes in incarceration rates than states with partisan judicial re-elections.

To do so, I estimate the average difference in *Incarceration Rate* in states with partisan elections compared to states with non-partisan elections across the range of *Public*

Punitiveness$_{t-3}$ in the states, based on the coefficients reported above in model 4-3. figure 4-4 is a marginal effects graph, where the Y-axis is the difference in fitted values for change in *Incarceration Rate* for states with partisan versus non-partisan election of trial judges. The X-axis is the actual rage of *Public Punitiveness$_{t-3}$.*

Contrary to the *Non-Partisan Incarceration Hypothesis (H2),* which predicts that states with non-partisan elections will be more punitive and responsive to public opinion than states with partisan elections, and the *Non-Partisan Election Public Opinion Interaction Incarceration Hypothesis (H7),* figure 4-4 show no significant difference between the two election types. If the hypothesis were correct, we would expect to see a partisan-elections effect line that is significantly lower than the non-partisan-elections baseline and that is flat or negatively sloped, indicating that as *Public Punitiveness$_{t-3}$* increases, states with non-partisan elections are more responsive to public opinion than states with partisan elections. That is not the result presented in figure 4-4. States with non-partisan elections are statistically indistinguishable from states with partisan elections, regardless of the level of punitive mood in the state. And although the partisan elections line is negatively sloped (on the contrary, a non-partisan line would be positively sloped), the overlapping confidence intervals mean I cannot reject the null of H7. There is simply no support for the hypothesis that non-partisan elections create an environment where judges are more responsive to public opinion than states with partisan elections.

Figure 4-4: Marginal Effects of Non-Partisan Elections, Contrasted with Partisan Elections, Conditional on Public Punitiveness



Note: Contrasts in fitted values on the Y-axis are based on the estimated coefficients in table 4-2. 95% confidence intervals. Contrast statistics and levels of significance are listed in the appendix, table A4-3. NPE=531 State-Years; PE=230 State-Years.

A Difference-in-Differences Analysis

Having presented no aggregate time-series evidence that non-partisan elections produce incentives for trial judges to be more punitive than their colleagues in other states, or that those non-partisan judges seemingly respond to those electoral pressures, I turn now to exploring whether or not an effect can be found in any of the states that altered their retention mechanism by testing H10, the

> *Judicial Reform Incarceration Hypothesis*: When a state reforms its judicial retention mechanism from partisan elections to non-partisan elections, the state should exhibit a higher incarceration rate.

In this exploration, I employ a more rigorous, quasi-experimental research design using difference-in-differences (DD) analysis (Angrist and Pischke 2009; Card and Krueger 1993).

To conduct a DD analysis, researchers must have time-series observational data that cover a period of time both before and after some sort of treatment. In addition, the analyst must have a control, or untreated, observation with similar time-series data. In their seminal work using DD, Card and Krueger (1993) estimate the effect of a minimum wage increase on hours worked by low-skill service employees by comparing the change in New Jersey (the treated jurisdiction) after that state raised its minimum wage to the trend in Pennsylvania (the untreated jurisdiction). Put into experimental terms, New Jersey was the treatment group while Pennsylvania was the control group. Any difference in post-treatment levels of the outcome are thus attributed to the treatment.

Recall from chapter 2 that six states changed the mechanism by which they retain trial judges over the course of my time-series data. Of those six states, four of them changed from partisan elections to non-partisan elections.[6] Arkansas changed from partisan to non-partisan elections in 2001, Georgia changed in 1984, Mississippi in 1995, and North Carolina in 1996.[7] My hypothesis is that non-partisan elections will be associated with higher levels of punitive criminal justice in the states than partisan elections, so a change

[6] The other two states to change retention mechanisms were New Mexico, which changed from partisan elections to uncontested retention elections, and Utah, which changed from non-partisan elections to uncontested retention elections.

[7] As of this writing, North Carolina has changed its retention mechanism back from non-partisan to partisan elections as of 2016, and Arkansas is currently considering a similar transition.

from partisan to non-partisan constitutes a treatment and permits me to test the effect of that treatment.

DD analysis rests on two assumptions: Rubin's Stable-Unit Treatment Value Assumption (SUTVA), a standard assumption for all causal inference that requires independence of units, and the parallel trends assumption (PTA), which requires that variation in the outcome variable pre-treatment be parallel in both the treatment and control groups. I can safely assume the state-level data meet SUTVA since the incarceration rates in one state are unlikely to be related to the incarceration rates in another state or to the policies adopted by another state, making the state-year observations independent.[8] Examining figure 4-5, the PTA, however, does not appear to be met considering the trend lines move close to convergence at some points while widening at other points in time, and even crossing one another at still other points. However, the national-level trends are unimportant for two reasons. First, methodologically, this project is not an explanation of national trends in punitiveness. I take methodological advantage of the state-to-state variation in punitiveness and judicial retention. Second, as the t-test results in table 4-3 report, the difference between the two lines in figure 4-5 are not statistically significantly different, so analyzing them would be imprudent. However, the differences in means reported for each of the four states indicates that periods of non-partisan elections were associated with higher incarceration rates than periods of time under partisan elections,

[8] The policy diffusion literature might seem to suggest that policy outcomes in one state might be affected by nearby states, and thus creating a violation of the independence assumption. However, this literature explores policy adoption in laggard states based on prior adoption by a leader state. The literature does not suggest that the policy in the leading state in any way affects the outcome in related policy areas in laggard—or non-adopting—states.

differences that are statistically significant at the 0.001 level. These significant t-test results provide some tentative support for the *Judicial Reform Incarceration Hypothesis* as the level of incarceration is significantly higher in the post-treatment period, i.e., the non-partisan years, in each state.

Figure 4-5: Natural, National Trends in Incarceration by DD Experimental Group

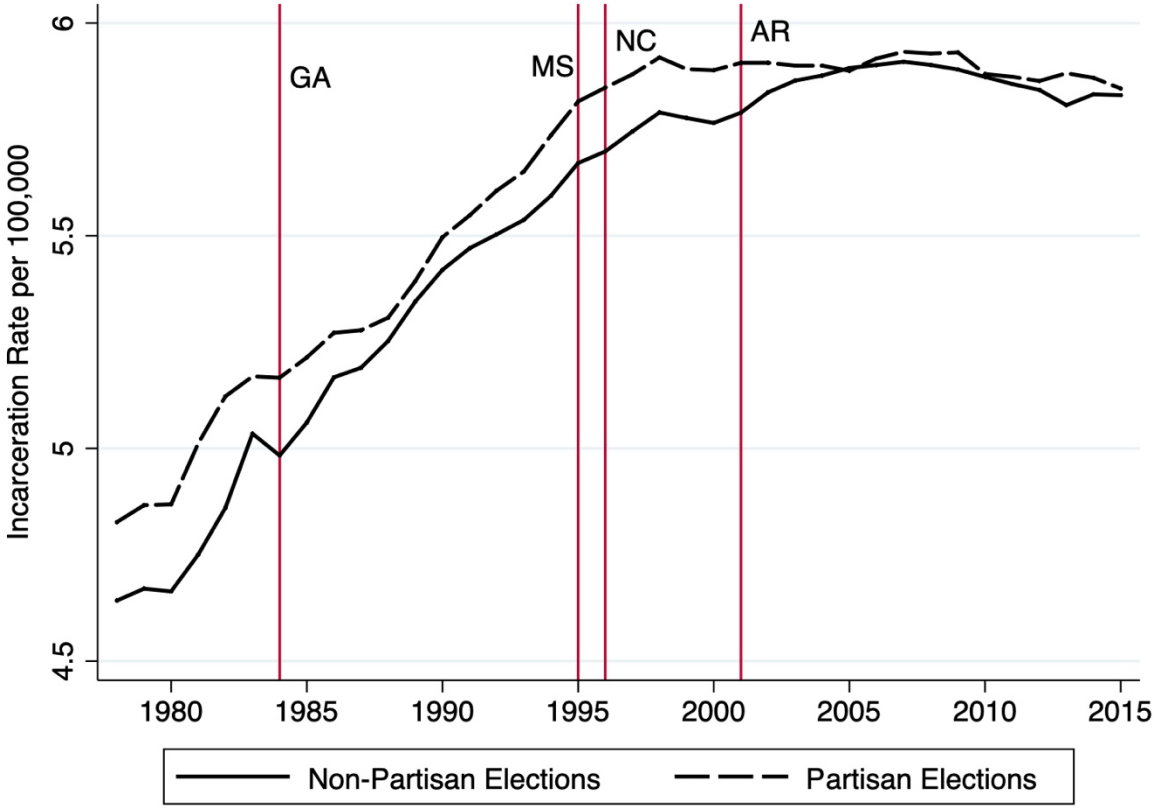


Table 4-3: T-Test Difference in Means, Incarceration Rate

	National	Arkansas	Georgia	Mississippi	North Carolina
Non-Partisan	5.53	6.15	6.08	5.95	6.00
Partisan	5.54	5.50	5.47	5.31	5.64
Difference (p-value)	–0.01 (0.790)	0.65 (0.001)	0.61 (0.001)	0.63 (0.001)	0.36 (0.001)

Fortunately, when the PTA is not satisfied, alternative statistical methods permit the creation of a synthetic control group that matches the treatment state on important characteristics (Abadie et al. 2010). I use the characteristics of the states in the regression model that vary by year to create the synthetic control groups for each of the four states. As demonstrated in figure 4-6, the PTA is satisfied with the synthetic control in three of the four states: Arkansas, Georgia, and Mississippi, as indicated by the nearly identical movement of both trend lines for these three states. The two trend lines in North Carolina do not move together and in fact cross one another at one point in time, thus violating parallel trends. I therefore limit my estimation of difference-in-differences to those three states where the PTA is met.

Figure 4-6: Parallel Trends for Treated and Synthetic Control States, Incarceration Rate

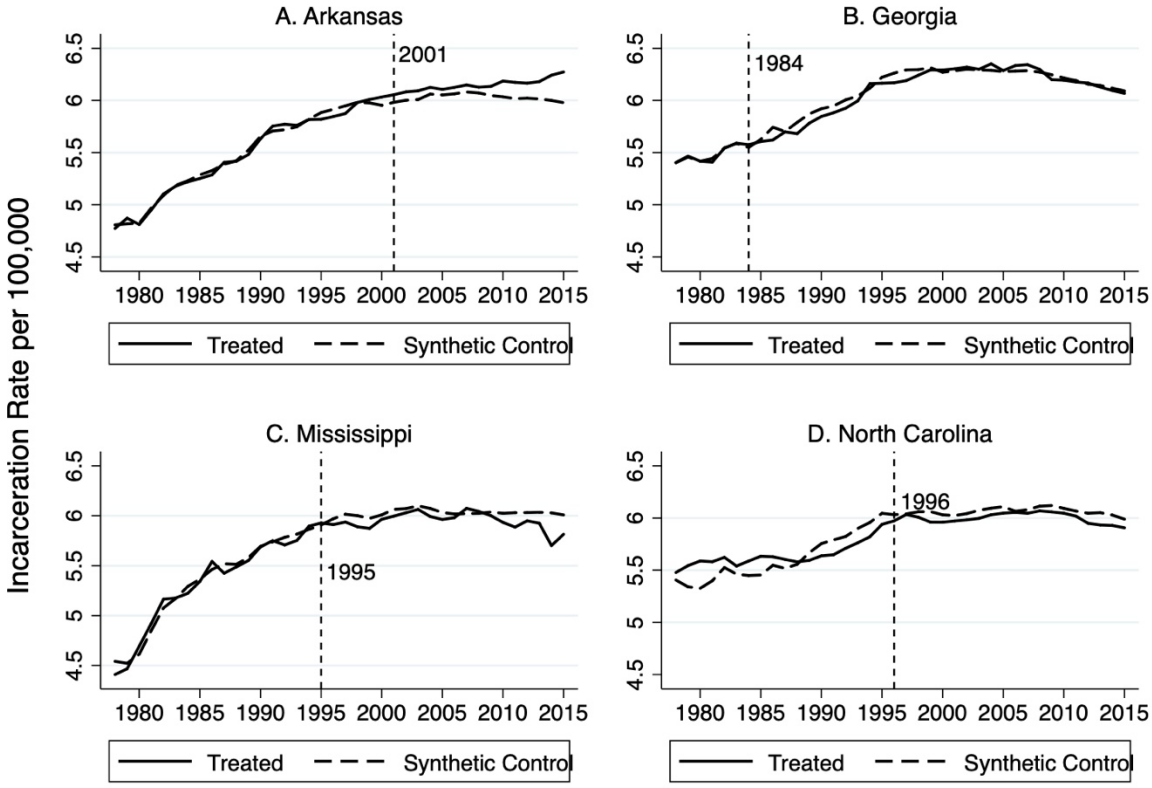


Table 4-4 reports the results of the three difference-in-differences analyses for *Incarceration Rate* for the three states that met the parallel trends assumption: Arkansas, Georgia, and Mississippi. A DD analysis was not conducted on North Carolina because, even with the synthetic group, the PTA was not satisfied; looking at the pre-reform trends in Panel D of figure 4-6, one can see convergence and crossover of the trend lines, a violation of the PTA.

The primary finding reported in table 4-4 is that no statistically significant difference-in-differences exist for *Incarceration Rate*. Compared to synthetically constructed control states, each of the states that changed their judicial retention scheme from partisan to non-partisan elections experienced no statistically discernable difference in incarceration rates as a result of the reform.

Recall from the time-series evidence presented above that no statistically significant differences existed between states with partisan elections and non-partisan elections for trial judge retention. The evidence from the difference-in-differences analysis is thus fully consistent with the time-series analysis, and leads us to the same conclusion: changes in judicial institutions from partisan to non-partisan elections do not appear to cause increases in state incarceration rates. Determining whether a change from no elections to some variant of elections might cause a change in incarceration rates is an unanswered question that would require different data and is beyond the scope of this project.

Table 4-4: Difference-in-Differences Estimation of Incarceration Rates following Judicial Reform

	Pre-Treatment	Post-Treatment	Difference
Arkansas (2001)			
Control	5.486	6.029	0.543
Treatment	5.481	6.148	0.667
Difference	–0.006	0.119	**0.124 (0.407)**
Georgia (1984)			
Control	5.476	6.107	0.631
Treatment	5.472	6.084	0.612
Difference	–0.003	–0.023	**–0.019 (0.890)**
Mississippi (1995)			
Control	5.318	6.023	0.705
Treatment	5.306	5.946	0.64
Difference	–0.012	–0.077	**–0.065 (0.647)**

Difference in differences analysis estimated using Stata 15.0's `diff` command. Means reported are for variable *Incarceration Rate*. Each state changed judicial retention mechanisms for trial court judges from partisan to non-partisan elections. Pre-treatment period means are calculated before and including the respective years of change; post-treatment period means are calculated after the treatment year. Factual trends are estimated for treatment states; statistically generated synthetic trends for each state are estimated as the control condition. Statistic of interest is the difference-in-differences, which is in bold with the p-value in parentheses.

Conclusion & Discussion

America has become the most carceral nation in the world. Governments in the US incarcerate more citizens per capita than any other country in the world. The United States leads the world in incarceration for several reasons: a higher level of criminal conduct (National Research Council 2014), the more punitive attitudes of the American public (Enns 2014, 2016), the large (and growing) number of offenses governments treat as criminal (National Research Council 2014), and the individualism so prominent in our culture (Olson 2004).

I offer another explanation in this chapter. The US is only one of a few nations in the world that elects judges, and the only one that does so on such a wide basis (Friedman 2009). And I have demonstrated that jurisdictions with elected judges, in some institutional arrangements, have larger annual changes in incarceration than do jurisdictions without judges who must face the public to retain their seat on the bench.

First, relative to states where judges are not retained via election, judicial retention elections in the states are associated with more responsiveness to public opinion. In all three types of elections, as the level of punitive mood among the public increases, the states have greater increases in their carceral population. Clearly, compared to more independent judges, trial judges who face election are responsive to public opinion on crime and punishment.

I have shown that both the incarceration rate is significantly lower in states with uncontested retention elections than they are in states where judges do not face election by the public, but only in states where the punitive mood of the public is low. In states with uncontested retention elections, as the level of punitive mood in a state rises, incarceration

rates rise to the point that they are not significantly different than states without elections at higher levels of punitive mood.

Similar results obtain when comparing non-partisan elections to states without retention elections. Relative to non-electoral states, in jurisdictions where judges face non-partisan elections to remain on the bench, incarceration rates are significantly lower at lower levels of public punitiveness. Once again, as punitive sentiment in a state rises, the incarceration rate in states with non-partisan elections also rises. Indeed, when punitive mood is above 0.58, non-partisan elections produce significantly greater incarceration rate changes than non-electoral states.

Furthermore, states with partisan retention elections are associated with significantly higher changes in incarceration than states without elections, but only at the upper end of the punitiveness scale.

Scholars frequently argue that elections provide a mechanism of accountability to the public for wayward judges (e.g., Bonneau and Hall 2009). Clearly, these results indicate that in most electoral arrangements, judges are indeed acting in a majoritarian fashion as predicted by my theory.

However, the comparison to non-electoral states is not the only relevant comparison. I also contrasted states with non-partisan elections to states with partisan elections but found no support for my hypotheses. Relative to states with partisan elections, states with non-partisan elections are statistically indistinguishable. What is more, contrary with my expectations, states with non-partisan elections are no more or less responsive to public opinion relative to states with partisan elections. Along the entire range of *Public Punitiveness*$_{t-3,}$ the confidence intervals for partisan elections overlaps the non-partisan

reference line, so there is no apparent responsiveness of either electoral type compared to the other.

From a representation standpoint, the results presented herein are fairly consistent. Electoral considerations apparently play some role in judicial behavior. Compared to non-electoral states, elections produce outcomes that are in accordance with public opinion, indicating that elections produce greater plebiscitary pressure than the absence of popular elections. From a normative standpoint, the results are also mixed. Indeed, some electoral systems are contributing to the mass incarceration problem in America, but the elected judges are apparently behaving in a majoritarian way, consistent with democratic theory. Given these results, the question remains: do judicial elections impact racial disparities in incarceration rates? What role does public opinion play in that relationship. These are questions that I explore in the next chapter.

Chapter 5: Elected Injustice: Judicial Elections, Public Opinion & Racial Disparity in Incarceration

Chapter Overview

Chapter 2 detailed a theory of judicial responsiveness to public opinion, in which I hypothesized that judicial elections should create incentives for judges in states with elections to be more responsive to perceptions of public opinion than judges in states without elections. In chapter 3, I introduced, validated, and explored a new dataset on public punitive attitudes, and in chapter 4, I tested several hypotheses about the relationship between judicial elections, public opinion, and incarceration rates. I reported that there is mixed evidence of judicial responsiveness to public opinion. Specifically, when compared to states without elections, judges who are elected appear more responsive to public opinion in that incarceration rates are higher for states where publics are more punitive and judges are re-elected rather than retained by some non-electoral mechanism. Moreover, as expected, incarceration rates are significantly more responsive to public opinion in states with non-partisan elections than in states with partisan elections.

In this chapter, I examine the impact of judicial elections and public opinion on a different outcome variable: the *racial disparity* in incarceration rates. I begin by discussing the association between race and crime in the minds of many Americans, both masses and elites, as well as the prevalence of racial disparities in criminal justice outcomes, which leads to several hypotheses that are tested using time-series cross-sectional (TSCS) and difference-in-differences analyses. Altogether, I find very little evidence that states with judicial elections differ significantly in their tendency to incarcerate Blacks more than Whites when compared to states without elections. I do find, however, that at lower levels of public punitiveness, non-partisan elections tend to produce greater racial disparities in

incarceration than partisan elections, which is at least partially consistent with expectations. Finally, I uncover evidence in one moderately punitive state—Mississippi—that racial disparities actually decreased following a change from partisan retention elections to non-partisan contests and discuss a potential explanation for why the evidence from this state is contrary to my expectations.[1]

Introduction

Previous research has demonstrated that incarceration rates in the United States have risen dramatically since the late 1970s, a trend I confirmed in chapter 4 of this dissertation. To make matters worse, racial minorities, especially Black Americans, bear the brunt of the distinctly punitive American criminal justice system. More Blacks than Whites in the US receive prison time for committing similar crimes under similar circumstances (Mustard 2001; The Sentencing Project 2013). Blacks receive longer sentences, on average, and they make up a disproportionate share of state prison populations (The Sentencing Project 2017; Yates 1997; Yates and Fording 2005).

That Black offenders and White offenders experience outcomes in the criminal justice system differently is indisputable. The question, then, is why? Criminology research points to two competing explanations. On the one hand, consensus theory contends that disparities in outcomes, such as arrest or incarceration, result from racial differences in criminal activity rather than discriminatory behavior on the part of official actors in the criminal justice system, i.e., the differential involvement hypothesis. Early scholarship concluded that incarceration disparity was a function of who commits crimes and

[1] Mississippi has an average score on *Public Punitiveness* of 0.586, about 20% of a standard deviation above the pooled mean for that variable of 0.579.

recidivates, not necessarily the harsher treatment of people of color (Hindelang 1978; Wilbanks 1987; Williams and Drake 1980). Lauritsen and Sampson (1998) summarize the position: "When restricted to index crimes, most individual level studies have shown that a simple direct influence of race on pretrial release, plea bargaining, conviction, sentence length, and the death penalty is small to nonexistent, once legally relevant variables (e.g., prior record) are controlled" (75).

On the other hand, conflict theorists argue that the criminal justice system—from the law itself (Stuntz 1998) to the actors enforcing it (Jackson 1989)—is fraught with bias and discrimination. In the view of conflict theory, racial disparities result from this systemic bias against Blacks and discrimination against them by actors from the police to the courts (Austin and Allen 2000; Spohn and Cederblom 1991; Spohn et al. 1981). Over time, research has moved sharply and definitively toward the conflict account of race and crime, as evidence of discrimination exists, but it is conditional on a variety of factors. Walker et al. (2004) conclude: "Pervasive evidence indicates that racial minorities suffer discrimination at the hands of the police … and within the court system" (358–9).

Beyond the sociological and criminological antecedents of racial disparity, recent research has started to investigate political explanations—such as support for Republican presidential candidates, Republican control over state government, and judicial conservatism—for racial disparities in incarceration (Keen and Jacobs 2009; Yates and Fording 2005). But little attention has been given to evaluate the role of judicial elections in contributing to racial disparities in punishment. Studying the policy implications of judicial selection mechanisms has a long tradition in the state courts literature (Brace and Boyea 2008; Brace and Hall 1995; Caldarone et al. 2009; Canes-Wrone et al. 2014; Canes-

Wrone et al. 2012; Crawford 2018), but that research has mostly focused on appellate courts (but, see, e.g., Nelson 2014; Rachlinski et al. 2009), and relatively little focus has been placed on racial disparities in the criminal justice system (but, see Park 2017). Given these shortcomings in the literature, I raise the question, do the institutional arrangements for trial judge retention in the states influence racial disparities in incarceration?

To answer this question, I draw from research in several disciplines—sociology, criminology, psychology, and the legal field, as well as studies of state court elections in political science. Research on judicial and other elite behavior provides a reasonable foundation for the claim that elections might influence judicial decision-making leading to racially disparate incarceration rates. Studies find that judges are influenced by myriad extra-legal factors, including their ideology (Segal and Spaeth 2002), public perceptions of the courts (Epstein et al. 2006), their race and gender (Haire and Moyer 2015), and even how recently they have taken a break to eat (Danziger et al. 2011). Electoral considerations that influence whether judges keep their jobs or not are surely an important factor weighing—either implicitly or explicitly—on the minds of jurists who must face voters (Canes-Wrone et al. 2014; Mayhew 1974), as they do in 38 states (National Center for State Courts 2018).[2] In other words, judges should respond strategically to the political and legal environment in which they must make decisions (Epstein and Knight 1998).

Prior research finds ample evidence for the proposition that judges are indeed subject to electoral considerations (Brooks and Raphael 2002; Canes-Wrone et al. 2014; Gordon and Huber 2007; Hall 2001; Huber and Gordon 2004), and that politically salient issues are strong motivators for elected judges (Caldarone et al. 2009; Canes-Wrone et al.

[2] See figure 1-1 in chapter 1.

2014; Canes-Wrone et al. 2012; Park 2017) in a way that non-salient issues are not (Canes-Wrone et al. 2018). One particularly salient topic in American politics is crime (Costelloe et al. 2009; Cummins 2009; Levitt 1997; Shugerman 2012; see also Democracy Fund Voter Study Group 2017). In fact, Shugerman (2012) argues not only that crime is a prominent political issue but that much of the movement surrounding the reform of state judicial selection institutions took on an explicitly anti-crime message. Elected judges may behave similarly to other elected officials who are aware of and speak to constituents' preferences toward crime (Enns 2014; Peffley and Hurwitz 2010; Petrocik et al. 2003). Americans tend to prefer punitive responses to crime, such as incarceration and capital punishment, rather than "softer" approaches like prevention and rehabilitation (Johnson 2001; see also chapter 3 of the current work). Incarceration rates, moreover, are positively correlated with, and are responsive to the punitive attitudes of the public (Enns 2014, 2016; Jennings et al. 2017; see again chapter 3).

Moreover, crime and race are inextricably linked in the minds of many Americans. Research has long demonstrated that Whites have associated Blacks with criminality (Correll et al. 2002; Eberhardt et al. 2004) and with violence (e.g., Hurwitz and Peffley 1992). Indeed, in the 2016 American National Election Survey, nearly four in ten respondents asked whether Blacks are violent agreed that they were.[3] Moreover, Whites' beliefs in Black stereotypes influences the policy preferences of White Americans on a range of policy, such as welfare (Gilens 1998; Hurwitz and Peffley 1992; Peffley et al. 1997) and health insurance and government services (Enders and Scott 2019), as well as

[3] The ANES question is on a 7-point scale, coded 1=Peaceful and 7=Violent. Of respondent offering a response to the question, 39.16% selected 5 or above, i.e., the "violent" end of the scale. Nearly as many selected the midpoint of the scale.

criminal justice policies, such as support for prison privatization (Enns and Ramirez 2018), support for drug searches by the police (Peffley et al. 1997), opposition to prison furloughs (Hurwitz and Peffley 1997), harsher sentencing (Hurwitz and Peffley 1997; Peffley and Hurwitz 2002), and support for the death penalty (Peffley and Hurwitz 2002, 2007; Peffley et al. 2017).

Trial court judges are subject to these biases as well. Research has demonstrated that elected trial judges in Kansas hand down longer sentences to Black defendants closer to electorally important dates, such as filing deadlines or election day (Park 2017; Rachlinski et al. 2009). Whether or not this occurs on a wider scale, across the country rather than in a single state, is an important question that I undertake in this chapter.

I use the NPS dataset which, as detailed in chapter 2 of this dissertation, provides state prison populations by race for all 50 states and data on judicial retention from the National Center for State Courts to estimate linear time-series regression models with panel-corrected standard errors (Beck and Katz 1995). As indicated, contrary to expectations, the retention mechanism for trial judges is mostly unassociated with racial disparities in incarceration. Indeed, I find no time-series evidence that electoral systems are associated with different levels of racial disparity than states with non-electoral systems for trial judges. However, I do find time-series evidence that non-partisan elections are, as expected, associated with greater racial disparity than partisan elections, but a causal analysis reveals that, in one state at least, changing from partisan to non-partisan elections produced a decrease, rather than an increase, in racial disparities, an anomaly that I examine more closely.

An Empirical Test

Chapter 2 details the theoretical foundation for this chapter. Recall, in summation, that (1) crime and race are salient issues in American politics; (2) the public, on average, prefers punitive, rather than rehabilitative, responses to crime; (3) most Americans in general associate Blacks with criminality; (4) judges are not immune from this bias; (5) incarceration rates are responsive to increased punitiveness in public opinion; (6) judges sentence Blacks to prison more frequently than Whites and (7) hand down harsher sentences to Black defendants than to White ones;[4] (8) judges who seek their seat on the bench via elections must be responsive to public opinion because (9) they are subject to the same electoral considerations as other politicians, namely the desire to win re-election. In addition, all things equal, elected judges may be less sensitive to the charge that tougher sentencing produces racial disparities in incarceration because most state publics are fairly punitive and state electorates are mostly White. In other words, their goal is not to create more racial disparities, per se, but to remain tough in their sentencing, even if racial disparities in prison populations consequently obtain.

Generally speaking, many of the hypotheses derived to predict the relationship between judicial retention procedures and changes in incarceration rates are likely to apply to racial disparities in incarceration, as well. Thus, I propose the *Elections Racial Disparity Hypothesis:*

> States with elections for retaining trial judges will have higher Black-to-White incarceration disparities than states without such elections.

[4] It follows from (6) and (7) that Black inmates should comprise a larger proportion of the incarcerated population than White inmates, and in many states, they do.

As noted earlier, however, not all judicial elections are alike. This analysis distinguishes between the three types of judicial elections in the states: uncontested retention, non-partisan, and partisan elections. I theorized that the party identification of candidates is the most important heuristic for American voters. However, when institutional arrangements prohibit that heuristic, as in the case of non-partisan elections, judicial candidates should (1) behave on the bench in a way that is responsive to the attitudes of the public (2) in order to have a ready-to-run campaign message that (3) gives voters a retrospective accounting of the judge's effectiveness on the bench and a reason to vote to return the judge to office. Thus, I test the *Non-Partisan Elections Racial Disparity Hypothesis*:

> States where trial judges re-elected in non-partisan elections will have higher Black-to-White incarceration disparities than states with partisan judicial elections.

When considering what role politics plays in public policy, it is critical to understand how public opinion might condition institutional design. In states where the public exhibits a high punitive mood, elections should exert the largest effect on racial disparities in prison populations. More formally, I test the *Public Punitiveness Interaction Racial Disparity Hypothesis*:

> More (less) punitive states with trial judges retained in elections will have higher (lower) racial disparities in incarceration rates in a given year than equally punitive states without judicial elections.

Data & Methods

To test my hypotheses, I use the NPS dataset (United States Department of Justice 2017) augmented with data from a variety of other sources. My dependent variable, which comes from the NPS, is the change in *Black Disparity* from the previous year.[5] *Black Disparity* is measured by first calculating the number of Black inmates per 100,000 Black population in the state and the number of White inmates per 100,000 White population in the state, then by dividing the first quotient (Black incarceration) by the second (White incarceration) (Yates and Fording 2005).[6] Figure 5-1 reports the Black-to-White disparity at the national level over time, showing a sharp and steady decline in racial disparities in prison populations beginning in 2001, while figure 5-2 shows the trend in each state.[7] I then measure the change from the previous year. Positive values indicate an increase in *Black Disparity* from the year prior, while negative values indicate a decline. Resulting values range from .28 (Wyoming, 1978) to 29.43 (Rhode Island, 2001). The unit of observation for the analysis is the state-year.

[5] See footnote 4 in chapter 2 and appendix figure A2-1 and table A2-1.
[6] See equation 2.1 in chapter 2.
[7] Table A5-1 in the appendix reports little over-time change in rank-ordering of the states on *Black Disparity*.

Figure 5-1: Annual Change in National Black Disparity in Incarceration, 1978-2015

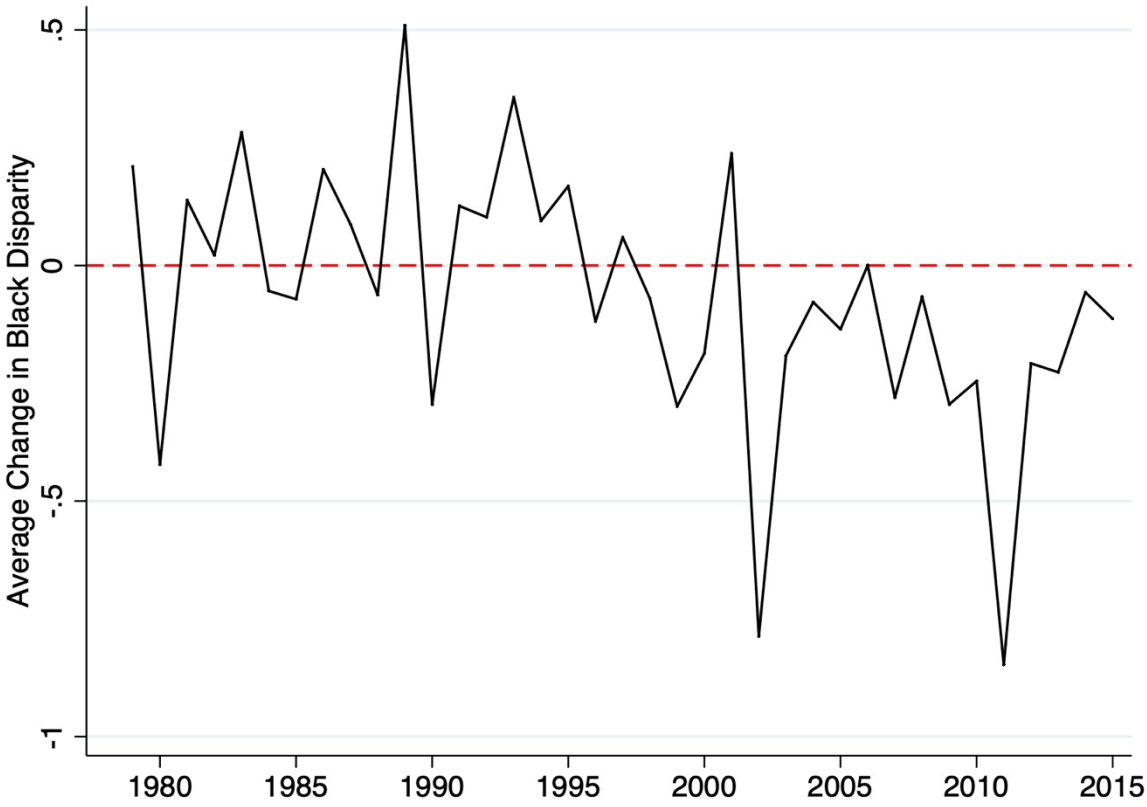


Figure 5-2: Black Disparity by State, 1978-2015

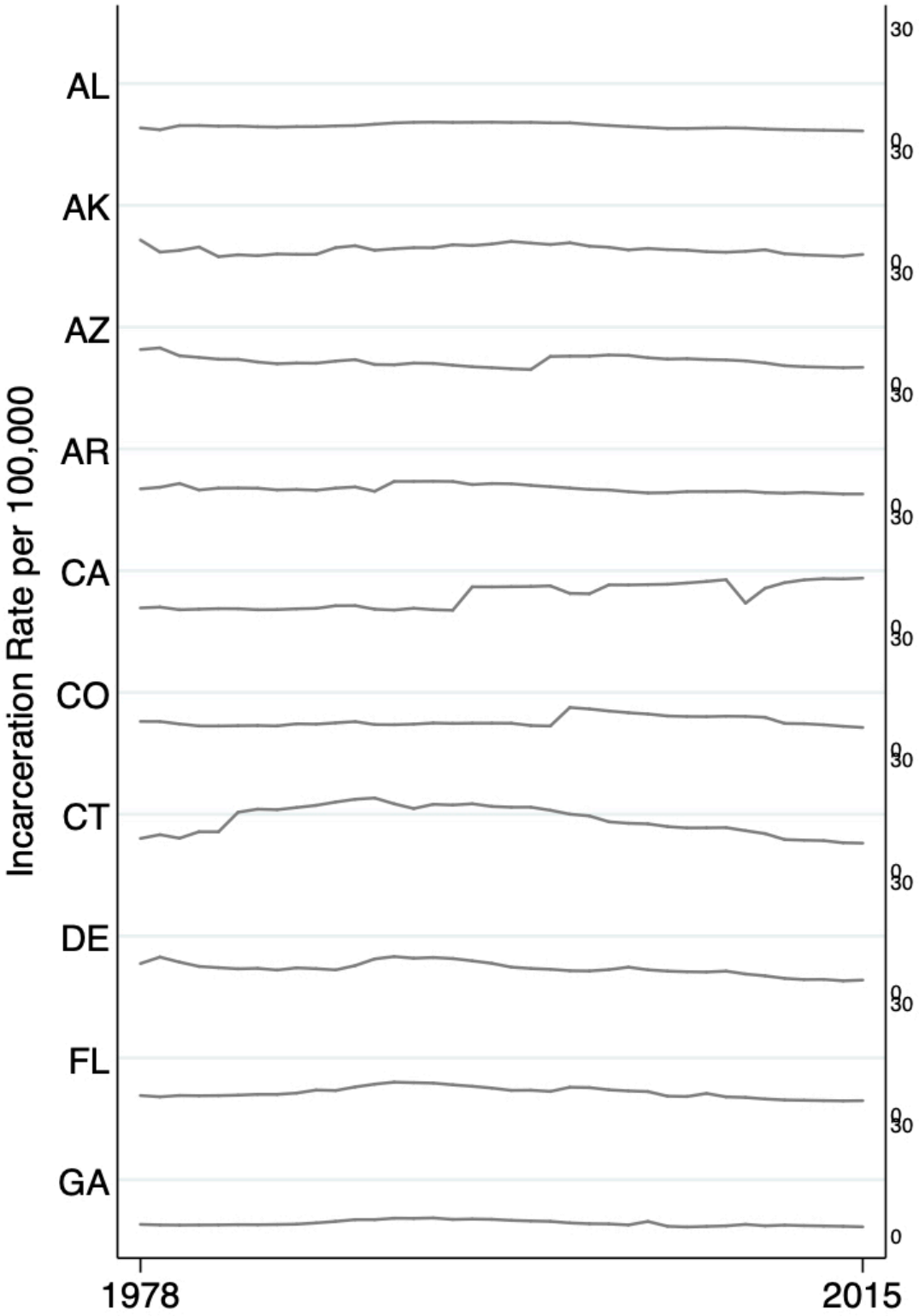


Figure 5-2: Black Disparity by State, 1978-2015, cont.

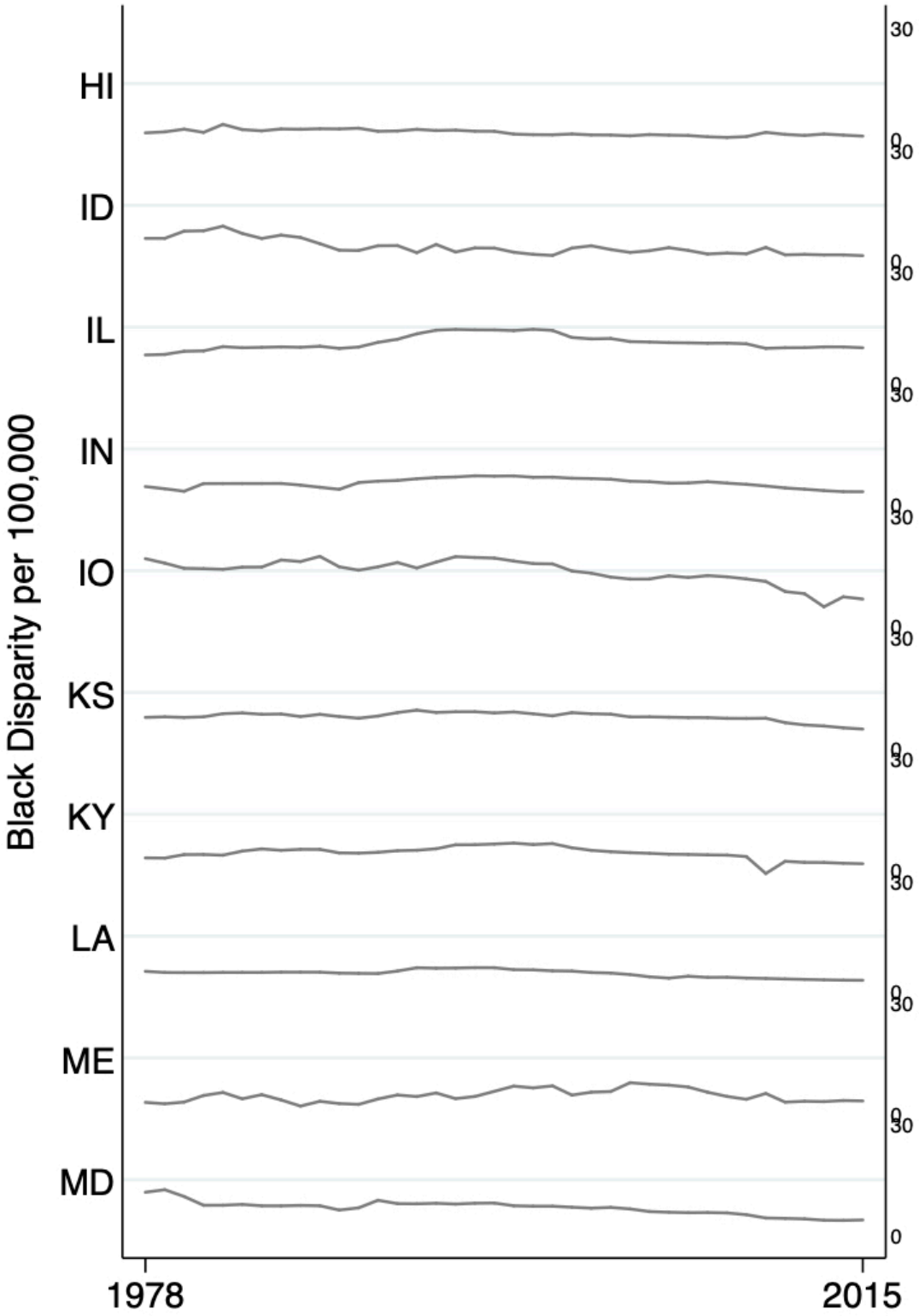


Figure 5-2: Black Disparity by State, 1978-2015, cont.

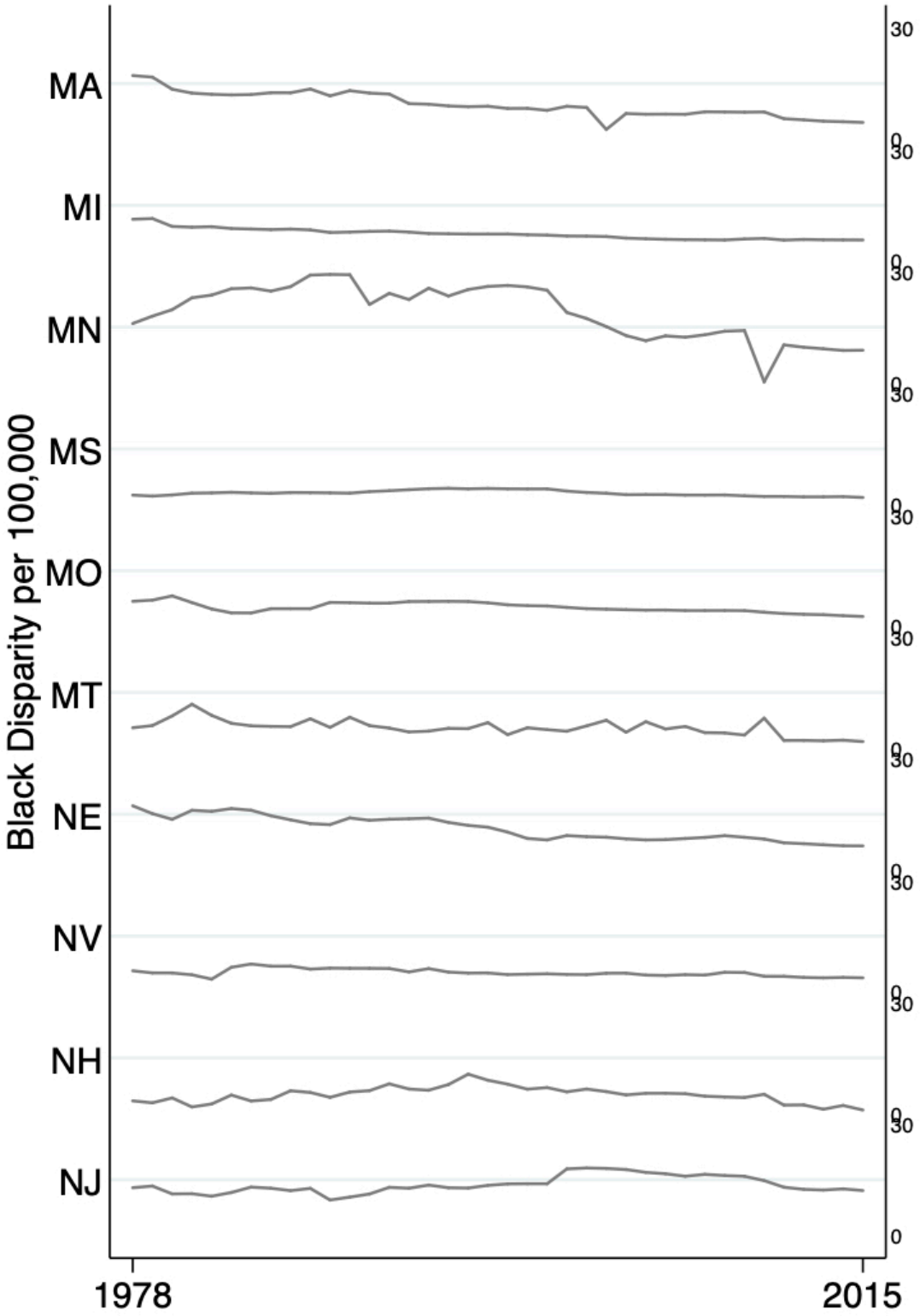


Figure 5-2: Black Disparity by State, 1978-2015, cont.

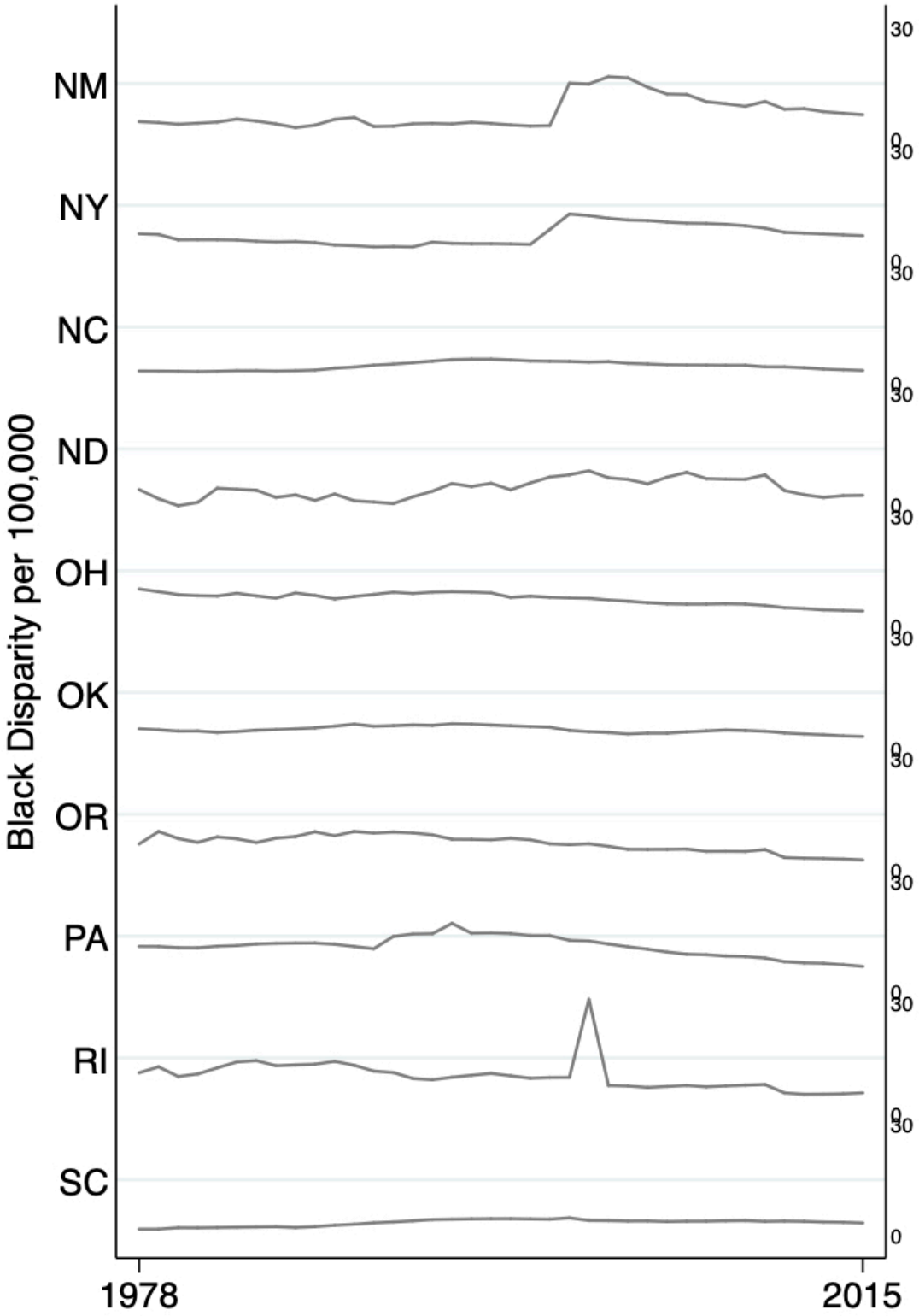
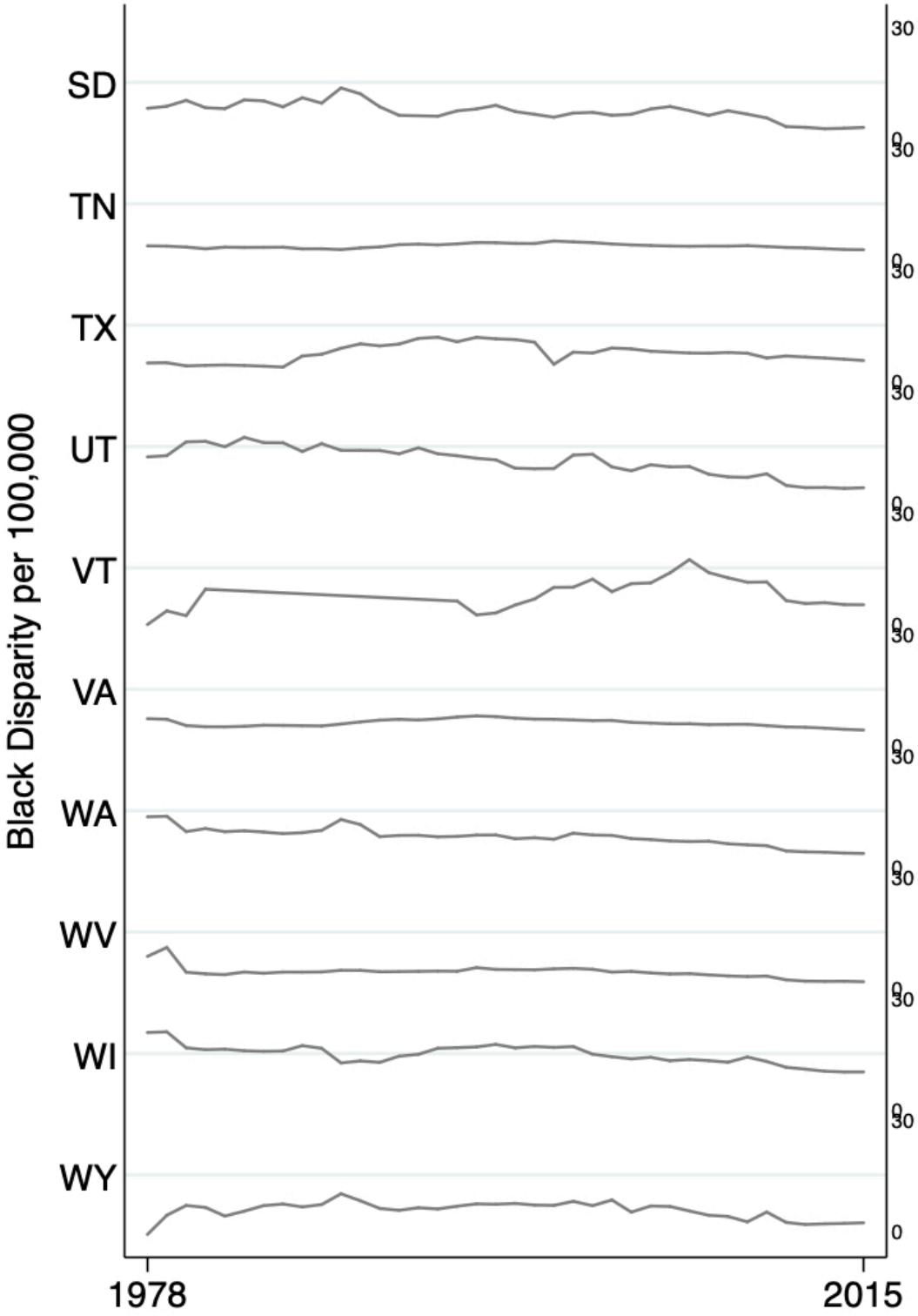


Figure 5-2: Black Disparity by State, 1978-2015, cont.



States are displayed in alphabetical order. The horizontal axis represents the year (1978-2015) and the vertical axis is the state's Black disparity in incarceration rate per 100,000 population; all vertical axes are scaled from 0-30 for direct comparability.

The main independent variable, *Retention Method*, is a categorical variable indicating the various mechanisms for retaining state trial judges (National Center for State Courts 2018). Each state-year in the dataset is coded with the process by which trial judges retain their seat on the bench: uncontested retention (coded 1), non-partisan (2), or partisan (3) election. States with no elections are coded as 0 and serve as the reference category. As before, four states with mixed systems, where some jurisdictions appoint judges while others elect them, are excluded from the analysis.[8] In the TSCS regression models, *Retention Elections* is operationalized as a series of indicator variables, so the coefficients for each election type indicate its relationship with *Black Disparity* compared to states without elections to retain trial judges. The second predictor of interest, in addition to *Retention Method*, is public opinion relating to crime and punishment, $Public\ Punitiveness_{t-3}$, described in chapter 3.

Each regression model also includes controls for a similar set of variables as the models in chapter 4. Table 5-1 reports the nine control variables, classified as political, legal, geographic-demographic, or economic variables.[9]

Table 5-1: Control Variables

Political Factors	Legal Factors	Geographic-Demographic Factors	Economic Factors
Judicial Term Length	Arrest Disparity$_{t-1}$	South	Income Per Capita$_{t-1}$
Recall	No IAC	Percent Black	
Elected Prosecutor		Percent Bachelor's Degrees	

[8] The four states are: Arizona, Indiana, Kansas, and Missouri.

[9] Recall from chapter 2 that the conflicting expectations on *Percent Black* led to the possibility of a curvilinear effect on the dependent variables, and in chapter 4, I found such an effect on *Incarceration Rate*. However, no such effect is observed on *Black Disparity*, so $Percent\ Black^2$ is not included in the models in this chapter.

I estimate the relationship between judicial retention mechanisms and racial disparities using a series of linear regression equations, since *Black Disparity* is a normally distributed continuous variable. As in chapter 4, because the data are time-series cross-sectional, OLS is inappropriate due to concerns about autocorrelation and heteroskedasticity. To correct for these problems, I use linear regression with panel-corrected standard errors (PCSE) and state-specific autocorrelation corrections (Beck and Katz 1995). As noted, the unit of analysis in each model is the state-year.

Model 5-1 in table 5-2 reports the coefficient estimates of a simple baseline model regressing the dependent variable, annual changes in states' racial disparities in incarceration, on the two primary predictors, judicial retention mechanisms and public punitiveness. The first observation to note is that racial disparity is apparently unaffected by the presence of judicial elections. While the coefficients for all three types of judicial elections are negative, none of the coefficients are statistically significant, meaning that disparities do not differ between no election and election states. The coefficient for *Public Punitiveness*$_{t-3}$ is positive, as expected, but it too fails to reach conventional levels of statistical significance.

Model 5-2 includes the nine control variables. As in model 5-1, the regressors of interest fail to reach statistical significance. Of the controls, *Arrest Disparity*$_{t-1}$ significantly predicts a change in *Black Disparity*. As the arrest disparity increases, incarceration disparity declines, indicating the displacement of the location of racial bias in the system rather than a lack of bias from judges. Additionally, as expected, states with a larger proportion of residents completing a bachelor's degree are associated with steeper declines in racially disparate incarceration rates. Thus far, I have found no evidence to suggest that

judicial elections, on average, lead to an increase in the racial disparity of a state's incarceration rate.

Table 5-2: Predicting Racial Incarceration Disparities based on Judicial Retention Mechanisms and Public Opinion

	Model 5-1	Model 5-2	Model 5-3
Reference Category is No Elections			
Uncontested Retention Elections (URE)	–0.02 (0.185)	–0.05 (0.187)	0.70 (2.949)
Non-Partisan Elections (NPE)	–0.04 (0.173)	–0.15 (0.129)	1.89 (2.429)
Partisan Elections (PE)	–0.04 (0.179)	–0.23* (0.127)	–0.28 (2.695)
Public Punitiveness$_{t-3}$	1.81 (1.584)	1.10 (1.839)	2.68 (3.966)
Δ Black Disparity$_{t-1}$	—	–0.25*** (0.077)	–0.25*** (0.077)
URE x Public Punitiveness$_{t-3}$	—	—	–1.30 (5.044)
NPE x Public Punitiveness$_{t-3}$	—	—	–3.52 (4.190)
PE x Public Punitiveness$_{t-3}$	—	—	0.05 (4.642)
Arrest Rate Disparity$_{t-1}$	—	–0.05** (0.022)	–0.05** (0.022)
No Intermediate Appellate Court	—	0.04 (0.117)	0.03 (0.228)
Elected Prosecutors	—	–0.14 (0.097)	–0.12 (0.096)
Judicial Term Length	—	–0.00 (0.006)	–0.00 (0.005)
Judicial Recall	—	–0.02 (0.119)	–0.04 (0.121)
South	—	0.09 (0.114)	0.08 (0.112)
Black Population	—	0.03 (0.050)	0.03 (0.050)
Bachelor's Degrees	—	–0.36** (0.141)	–0.36** (0.141)
State Income$_{t-1}$	—	–0.00 (0.000)	–0.00 (0.000)
Constant	–1.16 (0.930)	0.72 (1.263)	–0.21 (2.350)
N	1,273	1,219	1,219
R^2	0.005	0.074	0.075
Number of States	46	46	46

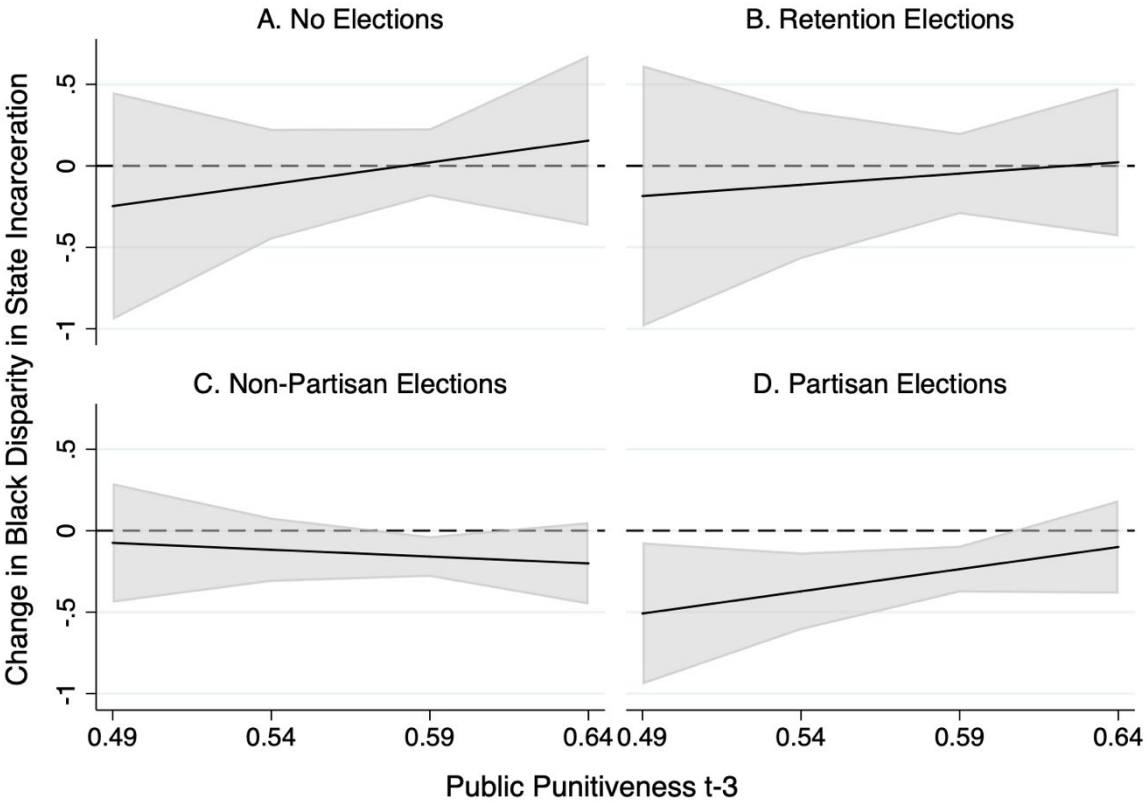
Models estimated using Stata 15.0's xtpcse command. Data from National Prisoner Statistics, 1978-2015, US Department of Justice, ICPSR Study 36657. DV is yearly change in a state's Black-to-White incarceration disparity rate. Panel-corrected standard errors in parentheses. Results of two-tailed tests of significance are: *p< .1 **p< .05 ***p< .01. Autocorrelation corrected using panel-specific AR(1). Unit of analysis is the state-year.

The Conditional Effects of Elections and Punitive Mood

My prediction is that the effect of judicial elections on racial disparities is conditioned by public opinion. Figure 5-3 displays the marginal effect of public opinion on the change in *Black Disparity* for each of the four retention mechanisms, computed from the results reported in model 5-3. Examining Panel A, which shows the effect of public opinion on *Black Disparity* in states without elections, the confidence intervals are wide enough that it is easy to see why, when subtracting the effect of one of the other mechanisms, there is no room for finding a significant contrasting effect.

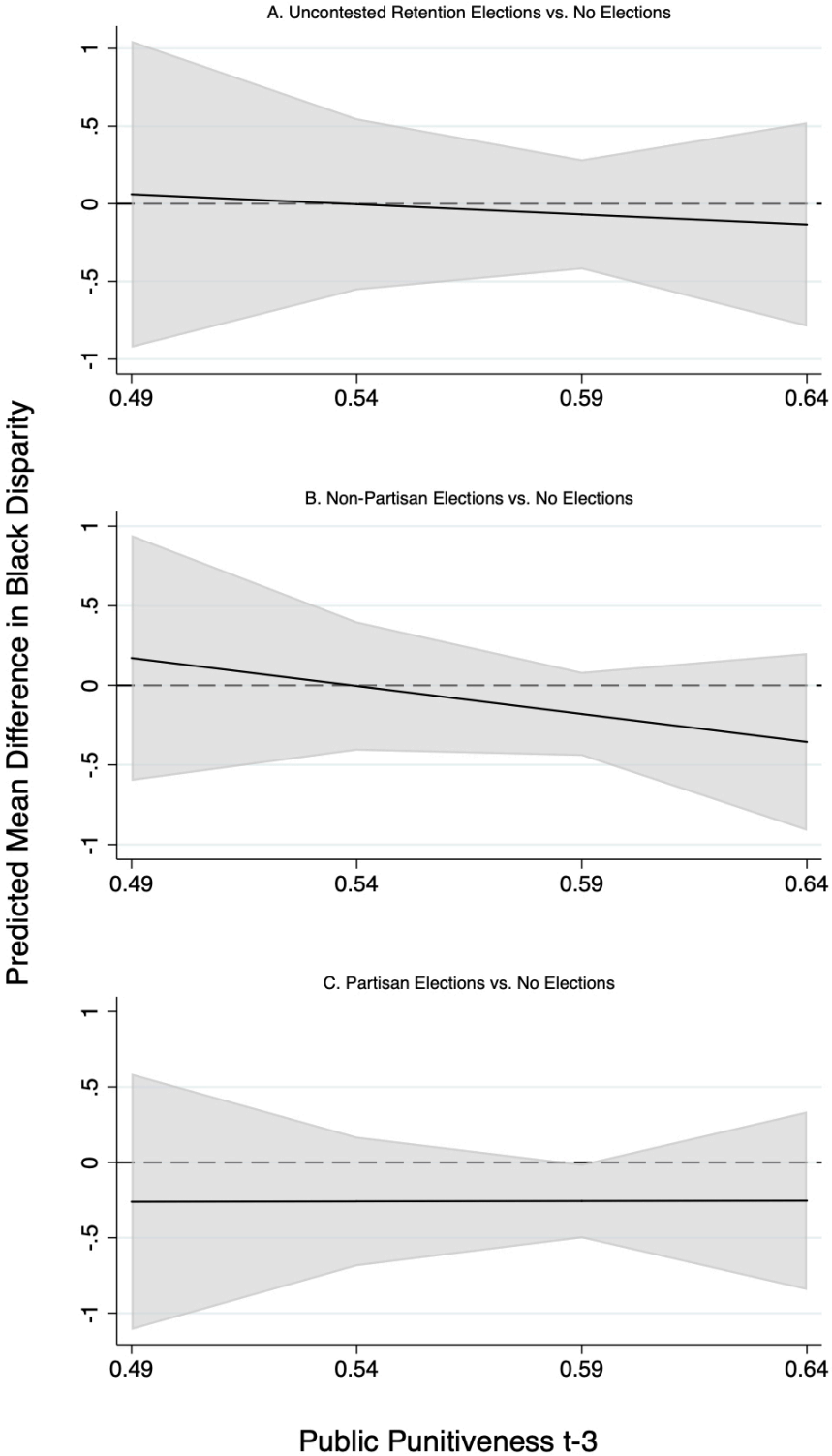
However, Panels C and D demonstrate that there is a significant impact of public opinion on racial disparity in states with either non-partisan or partisan elections. Further examination of these two panels shows that although public opinion is working in countervailing ways in these states, the confidence intervals are sufficiently narrow that a comparison of these two electoral systems is worthy of investigation.

Figure 5-3: The Impact of Public Opinion on Racial Incarceration Disparity, by Retention Mechanism



I test the *Non-Partisan Elections Racial Disparity Hypothesis* (H4) by including interactions between the three indicators of state judicial retention mechanisms and public punitiveness. Since interpreting the coefficients associated with interactions between a categorical variable (judicial elections) and a continuous variable (punitiveness) is notoriously difficult, I use the results in model 5-3 to compute the predicted contrast in *Black Disparity* rates for states with no elections versus each of three types of elections (displayed on the Y-axis in figure 5-4) across the range of state public punitiveness (on the X-axis), holding all other variables at their means. The actual range of $Public\ Punitiveness_{t-3}$, .49 to .64, is displayed on the X-axis.

Figure 5-4: Marginal Effects of Electoral Systems, Contrasted with Non-Electoral Systems, Conditional on Public Punitiveness



Note: Contrasts in fitted values on the Y-axis are based on the estimated coefficients from model 5-3 in table 5-2. 95% confidence intervals. Contrast statistics and levels of significance are listed in the appendix, table A5-3.

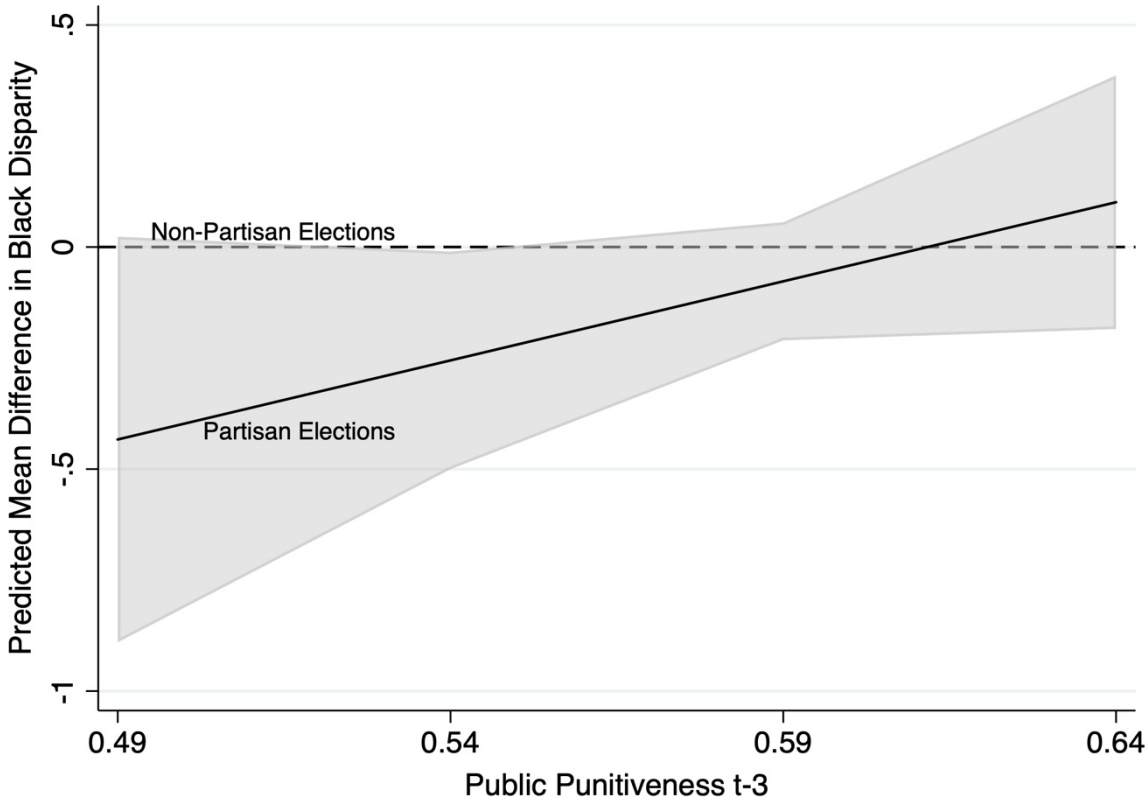
Figure 5-4 demonstrates that irrespective of the level of punitiveness expressed by a state's public, elections to retain trial judges do not produce significantly different levels of *Black Disparity* in state prison populations when compared to states with no elections. For all three graphs, the 95% confidence intervals overlap with the reference line (Y=0) at every point on the state public punitiveness scale, indicating there is no statistical difference in racial disparities across states with or without elections to retain trial judges.

Non-Partisan versus Partisan Elections

Thus far, I have reported no evidence that elections create an environment for trial judges to imprison Blacks at greater rates than they sentence Whites to prison. However, this comparison is to non-electoral systems. One remaining question is whether there is a difference between types of elections. Specifically, consistent with H4, the *Non-Partisan Racial Disparity Hypothesis*, do non-partisan elections produce greater racial disparities than partisan elections?

To test this hypothesis, I estimate the average difference in *Black Disparity* in states with partisan versus non-partisan elections across the range of public punitiveness in the states, based on the coefficients reported above in model 5-3. Figure 5-5 displays the marginal effects graph, where the Y-axis is the difference in fitted values of the dependent variable (i.e., the log of *Black Disparity*), for states with partisan (sloped line with 95% confidence intervals) versus non-partisan (reference line at Y = 0) election of judges. The X-axis is the actual range of *Public Punitiveness* at time *t-3*.

Figure 5-5: Predicting Racial Disparity in Prison Population by Selection of Judges across Racial Punitiveness, States with either Partisan or Non-Partisan Elections



Predicted values are estimated based on the results of model 5-3 in table 5-2. The Y-axis is the difference in fitted values of the dependent variable, the change in *Black Disparity,* for states with partisan versus non-partisan election of judges. The X-axis is the actual range of *Public Punitiveness* at time *t-3*. The solid sloped line with 90% confidence intervals is the estimated effect of *Public Punitiveness*$_{t-3}$ in states with partisan elections, contrasted with states with non-partisan elections, represented by the dashed reference line at Y = 0.

Inspecting figure 5-5, we see that in states with middle-range levels of *Public Punitiveness*$_{t-3}$, non-partisan elections yield significantly higher levels of racial disparities in incarceration than states with partisan elections, as indicated by the partisan elections line being lower than the reference line with confidence intervals that do not overlap the

reference line.[10] This result is consistent with the *Non-Partisan Elections Racial Disparity Hypothesis*. At higher levels of public punitiveness, however, racial disparities in incarceration remain higher in states with non-partisan versus partisan elections, as indicated by the rising slope, but the differences are no longer statistically significant. Thus, it seems that the tendency for judges elected in non-partisan elections to err on the side of not just higher levels of incarceration (chapter 4, figure 4-3) but also to incarcerate Blacks more than Whites (figure 5-5) is only true for states with mid-range levels of public punitiveness.

At higher levels of public punitiveness, partisan and non-partisan elections do not produce significantly different results, at least in terms of the tendency to incarcerate Blacks more than Whites. This finding makes sense. Nonpartisan judges produce more racially disparate incarceration rates than partisan judges only when partisan judges take signals from a less punitive electorate. When the state electorate is more punitive, however, partisan judges are just as likely as non-partisan judges to signal their toughness with racial disparate rates of incarceration. I turn next to exploring whether there is a causal relationship between judicial selection and disparities in incarceration in the four states that changed their selection mechanism from partisan to non-partisan elections.

A Difference- in-Differences Analysis

In chapter 4, I demonstrated that aggregate data can obscure state-by-state differences. Recall that while there was a significant difference between non-partisan elections and partisan elections in terms of the punitive policy outcomes produced, no

[10] Identical results obtain when Uncontested Retention Elections are moved into the baseline category. See figure A5-1 in the appendix.

causal relationship existed. In this chapter, we see little to no difference between the retention mechanisms in terms of producing differential racial disparities. However, the question becomes whether or not the aggregate data are obscuring causal relationships in one or more of the states. I answer this question again by conducting a series of difference-in-differences analyses (Angrist and Pischke 2009; Card and Krueger 1993).

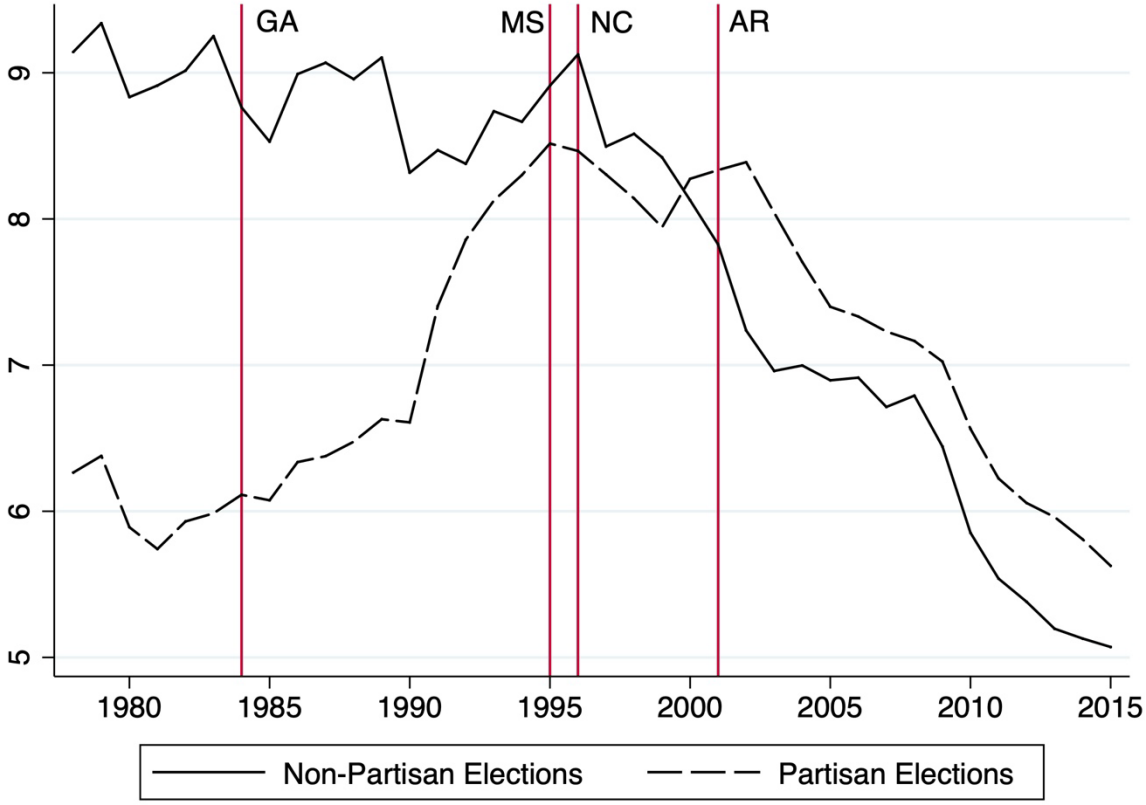
Recall that four states changed their retention mechanism from partisan to non-partisan elections. Georgia led the way in 1984, Mississippi (1995) and North Carolina (1996) trailed by about a decade, and Arkansas rounded out the changes in 2001. These changes permit me to test the *Judicial Reform Racial Disparity Hypothesis*:

> When a state reforms its judicial retention mechanism by changing from partisan elections to non-partisan elections, the state should exhibit an increase in racial disparity in incarceration rates.

Assuming again that the SUTVA is satisfied, the first analytical step in conducting a difference-in-differences estimation is inspecting the trends of the treatment and control groups. Recall that the trends for each group during the pre-treatment period must be parallel. I first look at the national level trends in figure 5-6. Although the levels of *Black Disparity* in the early years of the data are as expected—non-partisan elections were associated with higher levels than partisan elections—the trends are clearly not parallel. In fact, the levels were trending toward convergence, which they did around the year 2000 before the levels reversed and partisan elections became associated with higher levels of racial disparity than non-partisan elections.

However, the national-level trend may obscure state-level differences. This state-level variation in both *Black Disparity* and the judicial retention mechanism is the relationship with which I am most interested and that my data permit me to investigate.

Figure 5-6: Natural, National Trends in Black Disparity by DD Experimental Group



I begin exploring the difference in pre- and post-treatment levels of *Black Disparity* in these states with a student's t-test. Significant differences in t-test results would indicate that the reform in judicial selection mechanism brought about some change, although the results are not definitively causal. Table 5-3 reports the t-test results of differences in means of *Black Disparity* between non-partisan and partisan elections at the national level as well as for each state that changed its retention mechanism from partisan to non-partisan elections. The national level difference is in the expected direction and statistically significant. Of the four states that instituted a reform, two have statistically significant

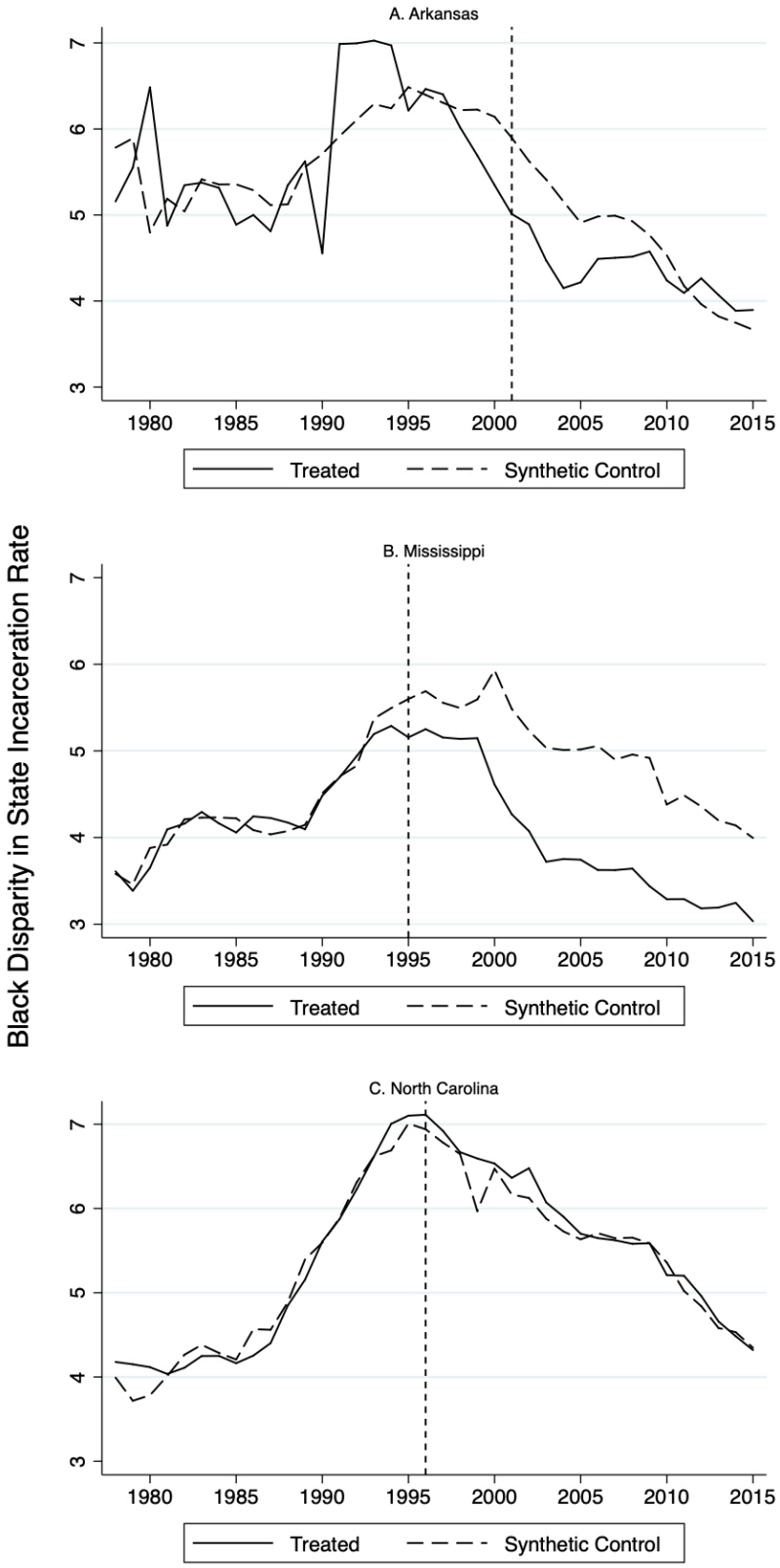
differences in means. The difference in Arkansas is in the opposite direction of expectations, with an average reduction of 1.43 in racial disparity. North Carolina had a statistically significant increase in mean post-reform, leading to an increase in *Black Disparity* of 0.76. The significant differences in means in these two states seem to indicate that the reform had a meaningful impact on *Black Disparity* in at least two of the states that warrants further investigation.

Table 5-3: T-Test Difference in Means, Black Disparity

	National	Arkansas	Georgia	Mississippi	North Carolina
Non-Partisan	7.80	4.30	4.35	3.98	5.78
Partisan	6.48	5.73	3.89	4.28	5.02
Difference (p-value)	1.32 (0.001)	–1.43 (0.001)	0.46 (0.134)	–0.30 (0.181)	0.76 (0.019)

To conduct that further investigation, I analyze the trends in the states separately with synthetic control groups (Abadie et al. 2010). I am unable to construct a synthetic control group for Georgia, however, because data on *Public Punitiveness* are not available for the pre-treatment years. Figure 5-7 displays the annual trends for each state and its synthetic control. Although the pre-treatment trends in Arkansas are somewhat rough, trends in all three states appear roughly parallel so I analyze all three.

Figure 5-7: Parallel Trends for Treated and Synthetic Control States, Black Disparity



Results from the difference-in-differences analyses of Arkansas, Mississippi, and North Carolina are reported in table 5-4. The statistic of interest is the bolded difference-in-differences statistic, with its associated *p* value, in the bottom right corner of each state's panel. The difference is not statistically significant in either Arkansas or North Carolina.

Table 5-4: Difference-in-Differences Estimation of Black Disparity following Judicial Reform

	Pre-Treatment	Post-Treatment	Difference
Arkansas (2001)			
Control	5.737	4.705	–1.032
Treatment	5.759	4.352	–1.407
Difference	0.022	–0.354	**–0.376 (0.201)**
Mississippi (1995)			
Control	4.293	5.002	0.709
Treatment	4.280	3.980	–0.300
Difference	–0.013	–1.022***	**–1.008 (0.001)*** **
North Carolina (1996)			
Control	5.010	5.681	0.671
Treatment	5.020	5.781	0.761
Difference	0.010	0.110	**0.090 (0.835)**

Difference in differences analysis estimated using Stata 15.0's `diff` command. Means reported are for variable *Black Disparity*. Each state changed judicial retention mechanisms for trial court judges from partisan to non-partisan elections. Pre-treatment period means are calculated before and including the respective years of change; post-treatment period means are calculated after the treatment year. Factual trends are estimated for treatment states; statistically generated synthetic trends for each state are estimated as the control condition. Statistic of interest is the difference-in-differences, which is in bold with the p-value in parentheses.

However, the change from partisan to non-partisan elections did have a causal impact on criminal justice outcomes in Mississippi. Results reported in the middle panel of table 5-3 indicate that the change of judicial retention mechanisms in Mississippi in 1995 resulted in a significant post-reform reduction in *Black Disparity*. Although this result is contrary to the retrospective voting theory I advocated in chapter 2, it is consistent with the

correlational findings reported above. In states with higher levels of punitive attitudes, non-partisan elections produced lower (albeit not statistically significant) levels of racial disparity. Mississippi's mean score on *Public Punitivness*$_{t-3}$ is 0.586, which is 20% of a standard deviation above the grand mean for the variable and right about the level at which the difference between partisan and non-partisan elections becomes statistically insignificant in the time-series contrasts reported in figure 5-5.

There is a probable explanation for this anomalous result. During the legislative debate on Mississippi's Nonpartisan Judicial Election Act of 1994, the legislative Black Caucus opposed the switch from partisan elections out of a fear that the reform would erase numeric gains made by Blacks in the Mississippi judiciary. To address this concern, reformers created more majority-black judicial districts and estimated that the number of Black trial judges in Mississippi would increase from six to between 15 and 20 (Becker and Reddick 2003, 31). Although this expected impact has, to the best of my knowledge, never been explored, if the number of Black trial judges did increase by two- or three-fold, a plausible impact would have been a reduction in Mississippi's racial incarceration disparity, even as the state switched from partisan to nonpartisan elections of trial court judges.

Conclusion & Discussion

Why do states differ in the degree to which incarcerated Blacks outnumber Whites compared to their share of state populations? The findings reported in this chapter lead me to reject one hypothesized explanation largely ignored in prior studies: the mechanism by which states retain trial judges. The overarching finding of this chapter is that no consistent relationship exists between the judicial retention mechanism and the level of racial

disparity in a state's carceral population. States that re-elect trial judges do not significantly differ in their levels of Black-to-White disparity from states where judges do not face re-election. Regardless of the type of election used to retain state trial judges, those states with elections produce racial disparities that are not different than states with non-election systems.

However, I provided evidence that while elections may not matter compared to non-electoral systems, the type of election does matter for racial disparities in incarceration. I presented evidence to support the hypothesis that non-partisan elections were more responsive to public opinion and increased racial disparity when compared to partisan elections. Non-partisan elections were associated with greater racial disparities, but only in states with lower levels of public punitiveness. As public punitiveness increased, however, any difference in racial disparities between partisan and nonpartisan elections became statistically insignificant.

Moreover, I presented evidence from a causal analysis that, in at least one state, reforming the mechanism of judicial retention from partisan to non-partisan elections caused a significant decline in Black-to-White prison disparities. Following this change in Mississippi in 1995, the state experienced a significant drop in the racial disparity of prison populations. Although the post-reform mean was still quite high at almost four Black prisoners for every White prisoner, it was still almost an entire standard deviation below the national mean for the post-1995 years. In the other two states—Arkansas and North Carolina—for which I estimated difference-in-differences analyses, I found no significant change in the post-reform era relative to synthetic control groups.

The results presented herein are hopeful, but only marginally. The findings of this chapter reveal that states that rely on elections to retain trial judges are either less likely to have higher racial disparities in their prison populations, or there is no difference in those states, when compared to states without re-election institutions. That judicial elections produce no difference in racial disparity, at worst, and, at best, a *decrease* in racial disparities in incarceration rates is promising.

Still yet, though, racial disparities persist. Clearly, racial disparity exists throughout the criminal justice system, but just because judicial elections and public opinion play little role in shaping them does not mean we should stop attempting to explain and correct the disparities. By the time cases get to the sentencing stage, the racial effects of sentencing outcomes may depend less on judicial attitudes or strategic behavior and more on systemic influences that moved a particular case to that stage of the criminal justice system.

Chapter 6: Conclusions

One constant lesson of the analyses in this work is that public opinion plays an important role in shaping outcomes in the criminal justice system, especially as it conditions the impact of institutional design. The ultimate policies produced by the judiciary—incarceration and incarceration disparities—are affected by both public opinion and by judicial elections. Although these policies are not affected in the same way in every state, their impact is consequential, not only for the incarcerated citizens and their families, but for every member of the populace and for the elites and institutions that govern the masses.

I have also demonstrated a consistent pattern of the impact of judicial design in the states. How a state determines its answer to the question, "to whom, if anyone, should judges be accountable?" impacts whether or not the state decides to elect its trial judges or not. And my results indicate that they type of electoral mechanism they opt to use matters for criminal justice policy. When judges are elected, and thus accountable to voters, public opinion shapes how judges appear to view their role on the bench and thus impacts incarceration rates and, to a lesser extent, racial disparities in incarceration.

Review of Findings

Public Opinion and Incarceration Rates

In chapter 3, I examined close to one hundred public opinion surveys and dozens of survey items dating from 1981 to 2016 to generate an estimate of public mood as it relates to criminal punishment in each state for each year. By creating a dynamic estimate of public punitiveness that varies both over time and across states, my measure of punitive mood overcomes limitations of previous measures, which varied either temporally or

across states, but not both. Using these new data, the Punitive Attitudes Dataset for the States, I demonstrated that state incarceration rates are responsive to public opinion. Higher levels of punitiveness among state publics translated into higher incarceration rates three years later.

Public Opinion, Judicial Elections, and Incarceration Rates

Building on the findings that public opinion affects punitive policy outcomes, I set out in chapter 4 to evaluate how public opinion conditions institutional arrangements. Specifically, I hypothesized that judicial retention mechanisms should be conditioned by public punitiveness such that states with judicial elections should be more responsive to public opinion than states without elections to retain trial judges. Through time-series regression analyses, I demonstrated support for my hypotheses. Compared to states without elections to retain trial judges, states with judicial elections are indeed more responsive to public opinion and, in some instances, that translates into greater incarceration rates. As the level of public punitiveness increases in states with uncontested retention elections, non-partisan elections, or partisan elections, the annual change in incarceration also increases, compared to non-electoral states.

I also explored the differences between non-partisan and partisan states using both time-series and difference-in-differences analyses. In both analyses, I found no support for the notion that one or the other institutional arrangement creates a greater plebiscitary pressure on incarceration than its counterpart. In the pooled time-series analysis, the contrast between the two was not statistically significant, indicating that partisan and non-partisan elections did not differ in terms of their impact on incarceration rates. In the difference-in-differences analyses, I investigated the impact of reform in three states where

the retention mechanism changed from partisan to non-partisan elections and obtained results similar to the time-series analysis. Such reform produced no significant changes in incarceration rates in the three states analyzed.

These results are different from previous research that found a greater plebiscitary pressure on non-partisan judges in criminal justice policy (see, especially, Canes-Wrone et al. 2014). This difference begs the question why my results diverge from such studies. Two explanations may account for the differences. First, as I noted in previous chapters, much of the scholarship on judicial elections has explored their effects on appellate courts, such as state supreme courts. It is possible that elections exert different pressures on state supreme court justices than they do on trial judges. A second possibility is that my research design did not lend itself to offer a definitive test of the theory. As I discuss later in this chapter, my theory is about individual judicial behavior, but I test that theory at the state level. Future research can attempt to adjudicate between these possibilities and determine whether trial judges simply behave differently than appellate judges.

Public Opinion, Judicial Elections, and Racial Incarceration Disparity

Having theorized that public opinion and judicial institutions should play a role in criminal justice policy, and that the former conditions the latter in shaping state incarceration rates, I turned in chapter 5 to explore the antecedents of racial disparities in state incarceration rates. The results presented from the time-series analysis is tentative at best. I found no indication that public opinion or judicial elections impact racial disparity in any consistent way. When comparing states with judicial retention elections to states without popular election, I found no significant differences in levels of racial disparity in prison populations, regardless of the level of punitiveness among the mass public. Previous

research on the politics of racial disparities in incarceration (e.g., Yates and Fording 2005) did not consider the role of judicial institutions, such as elections, instead focusing on judicial ideology and the partisan composition of state government, along with the ideological leanings of the public. They also reported that public opinion played no significant role in shaping Black disparity, consistent with my findings.[1]

I turned then to explore the differences in racial disparities in states with partisan versus non-partisan judicial elections. Although there is some evidence that public opinion in states with partisan or non-partisan elections plays a role in shaping racial disparity, the effects are the opposite of those predicted. As public opinion became more punitive, racial disparities in states with non-partisan elections declined, although the effect is significant only at moderate ranges of punitiveness (see panel C of figure 5-3). Meanwhile, racial disparity rose in states with partisan elections as punitiveness increased (see panel D of figure 5-3). In addition to the theory predicting the effect observed with partisan elections, some evidence exists that partisan elections produce more conservative judges (Bonica and Sen 2017), which may help explain that partisan elections produce greater responsiveness. However, it still leaves open the question of why partisan elections are associated with *lower* racial disparity. In terms of racial disparity, partisan elections produce greater policy responsiveness than non-partisan elections but reduce racial disparity. Thus, non-partisan elections, at least in some states with moderate levels of punitive public mood, produce significantly higher racial disparities than partisan states, which is consistent with my hypotheses.

[1] I also test the effects of partisan composition of government on racial disparity and found that explanation wanting as well. My results, in table A5-5 in the appendix, show that Democratic control over state government does not influence racial disparity.

Although I found little time-series support for the prediction that elections would be associated with greater racial disparity, I did find some differences that made exploring causality worthwhile. Because the time-series evidence pointed to racial disparities being higher in some non-partisan states, compared to equally punitive partisan states, I conducted difference-in-differences analyses with synthetic control groups in three states that changed from partisan elections to non-partisan elections expecting to find an increase in racial disparity post-reform. Instead, I found no evidence of a causal change in two of the three states. Racial disparities were statistically unchanged by a reform in judicial retention mechanisms. In the third state, Mississippi, I reported evidence that racial disparities actually decreased post-reform, a result I attribute to the increase in Black jurists associated with the reform.

Hypotheses Review

How do the above findings stack up against the hypotheses generated in chapter 2? Table 6-1 summarizes the hypotheses and whether each one was confirmed or rejected by the results presented in the preceding chapters.

As reported in table 6-1, many of the hypotheses turned out to be correct, while I was unable to reject the null hypothesis on others. Why is that the case? Several possibilities exist. One extreme possibility is that the theory is wrong. Perhaps trial judges, even when elected, do not consider public opinion to the extent that a theory such as mine predicts. However, I do not suspect that is the case for at least two reasons. First, some of the results presented provide evidence in support of the theory. Of course, these results may be spurious, but they hold fairly consistently across multiple robustness checks presented in the appendix (table A4-4 and table A4-5), so I am left to conclude that the

results are accurate and that they provide support for the theory. A second reason to be suspicious of a claim that the theory is wrong is because of its firm foundation in both democratic theory and in the strategic theory of judicial behavior. Evidence supporting the validity of both of these theories is ample, so I do not suspect my theory is flatly wrong.

Table 6-1: Summary of Hypotheses and Findings

Hypothesis	Confirmed or Rejected
Elections & Public Opinion Main Effect	
H1. Elections Incarceration Hypothesis	Partially Confirmed
H2. Non-Partisan Elections Incarceration Hypothesis	Partially Confirmed
H3. Elections Racial Disparity Hypothesis	Rejected
H4. Non-Partisan Elections Racial Disparity Hypothesis	Rejected
H5. Public Opinion Hypothesis	Confirmed
Elections & Public Opinion Interactions	
H6. Public Opinion Interaction Incarceration Rate Hypothesis	Confirmed
H7. Non-Partisan Election Public Opinion Interaction Incarceration Hypothesis	Rejected
H8. Public Opinion Interaction Racial Disparity Hypothesis	Rejected
H9. Non-Partisan Election Public Opinion Interaction Racial Disparity Hypothesis	Partially Confirmed
H10. Judicial Reform Incarceration Hypothesis	Rejected
H11. Judicial Reform Racial Disparity Hypothesis	Rejected

A second possibility for explaining the mixed—and especially the null—results is measurement and analytical problems. Most of the flatly rejected hypotheses are related to racial disparity, so perhaps public punitiveness alone is not a driving force explaining disparities in incarceration. Alternatively, it would be wise to consider the impact of other racial attitudes, such as racial resentment or racial attributions (Peffley et al. 2017) and how

such attitudes condition racial incarceration disparity. Analytically, it is also possible that my research is ecologically flawed. As I discuss in more detail below, I apply an individual-level theory to analyses of states. Perhaps the aggregation biased against finding significant results. I discuss additional shortcomings, along with future research prospects, later in this chapter.

Implications for Policymakers

The constant theme in the empirical results is that the impact of public opinion and judicial elections is not consistent. In some instances, public opinion plays a significant role in shaping criminal justice outcomes while in others, public opinion is moot. Criminal justice outcomes are shaped by some types of judicial elections within some ranges of public punitive mood, but not at other levels. Still yet, the results have implications for both institutional design and for criminal justice policy.

Institutional Design

Policymakers in several states recently have, as of this writing, proposed changes for their state's selection and retention of judges. In 2019, for example, five states introduced reforms related to judicial selection. In Arkansas, Representative Robin Lundstrum filed HJR1006 to change judicial elections from non-partisan to partisan, seeking to reverse that state's 2001 reform. Also seeking to change from non-partisan to partisan elections, Kentucky Representatives Myron Dossett and Jim Gooch Jr. filed HB123 in the Kentucky General Assembly, while in South Dakota, Senator Stace Nelson and Representative Tom Pishke filed SJR3. Pursuing a move in the opposite direction, New Mexico Senator Mark Moores filed SJR12 to change from partisan to non-partisan elections. And during the 2020 legislative session, Utah Senator Daniel McCay filed SJR8

to change from a merit system (i.e., elite appointment followed by periodic uncontested retention elections) to non-partisan elections. Despite these efforts, none of the legislation made it past committee referrals.

Policymakers clearly are still grappling with the questions raised in chapter 1. To whom, if anyone, should judges be accountable? To that complex question, a corollary question is: if judges are to be held accountable, to whom and how should that accountability be accomplished? As policymakers strive to answer these questions, the research I have just presented is instructive for institutional design.

If policymakers—and the public itself—desire a judiciary that is responsive to public opinion, at least in the criminal justice domain, elections apparently produce the desired outcome. As I reported in chapter 4, compared to states without elections, as punitive mood increased among the public, states with judicial elections increased their prison populations (see figure 4-3). However, because no states had changed from no elections to elections, I could not determine whether such a reform would create (i.e., cause) the responsiveness I observed in the time-series analyses. That empirical question remains unanswered for now.

We know, observationally at least, that elections for trial judges are associated with policy responsiveness in incarceration rates. That brings us to the question of whether the type of election matters. Evidence presented in chapter 5 shows that partisan elections, while producing responsiveness, are associated with *lower* levels of racial disparity in incarceration, likely for reasons I theorized in chapter 2, that (Republican) judges can fly under the radar by being somewhat less disparate in their treatment and relying on their party identification on the ballot to convey to voters that they are tough on crime.

Meanwhile, non-partisan elections produce countermajoritarian results in terms of racial disparity; as punitiveness increases, racial disparities decline (see figure 5-3). The difference is statistically significant for only a small range of punitiveness, but that range includes more than 60% of the observations in those states. Perhaps the level of punitiveness does matter, but there simply are not enough observations outside the range to find significant differences. The difference in Black disparity between these election types is present only in mid-range levels of punitive mood, where non-partisan elections are associated with higher racial disparities. And no difference between the two systems exists in the production of incarceration rates (e.g., figure 4-4). Thus, if policymakers desire responsiveness without raising questions of racial bias, partisan elections are apparently the most appropriate mechanism for judicial selection.

Policymakers must of course bear in mind the policy preferences of the public when making these decisions. Recall that public opinion conditions the judicial retention mechanism such that the impact of elections is likely to be different based on the level of punitive mood in the public.

Lastly, policymakers considering a reform must consider whether the reform itself will bring about the expected outcomes or whether unintended—or unexpected—consequences will result. I have presented no evidence that any recent changes in judicial retention has caused an increase in incarceration. The only quasi-experimental analysis in this research that produced any evidence of significant change showed a decline in racial disparity. Such a result is evidence of the countermajoritarian function of the courts, but likely obtained because of an increase in the number of Black trial judges rather than from

the reform in selection procedures itself. Policymakers thus must also consider mediating factors that follow judicial reform that might help bring about the desired outcome.

Criminal Justice Policy

Reforming the institutional design of the courts is not the only means to achieve desired policy outcomes. Policymakers also have recourse to enact reforms in criminal justice legislation. At least three avenues of change are available: reforming criminal law and arrest procedures, constraining prosecutorial decision-making, or constraining judicial decision-making through statutory means.

The evidence presented in chapters 4 and 5 showed that, regardless of the mechanism of judicial selection or the level of public punitiveness, when controlling for other factors, arrest rates played a significant role in shaping incarceration and racial disparities. This is to be expected. However, if policymakers view mass incarceration and racial disparities as problematic for social justice reasons as Democrats do, or for fiscal reasons as Republicans do, reforms to criminal law and arrest procedures should be in order. For example, lawmakers could expunge some criminal offenses, such as nonviolent drug possession, from the law books or reclassify some criminal statutes to a non-arrest status meaning law enforcement would have fewer crimes for which to arrest and book suspects.

After an arrest is made, prosecutors are empowered to make charging decisions. Criminal procedure could be reformed to force prosecutors to consider pre-trial options in some cases based on facts of the case or other mitigating factors related to the defendant such as age, prior criminal history, familial circumstances, socioeconomic background, etc. Providing criminal defendants with a second (or maybe even a third or fourth) chance to

get out of their circumstances and turn their life around would serve to reduce incarceration rates and remedy the downstream consequences of a life of cyclical incarceration.

A second path for prosecutorial reform is selection reform. Evidence presented in chapter 4 showed that incarceration rates are significantly higher in states with elected prosecutors (c.f. table 4-2). The theoretical motivation and mechanism behind this finding is likely the same as for elected judges: elected prosecutors are likely to appeal to a punitive public in electoral campaigns in order to secure sufficient support for re-election by proving they are tough on crime. Certainly, many day-to-day decisions are made by assistant prosecutors, but the chief prosecutor sets the tone and agenda for the office, and, at least according to the results presented in chapter 4, appointed prosecutors may set a less punitive tone or agenda than elected prosecutors. Policymakers might thus also consider electoral reform for prosecutors to reduce incarceration rates.

Finally, policymakers might reconsider sentencing guidelines and mandatory minimum sentences. Such laws were passed to ensure that judges are tough on crime by reducing their discretion to consider mitigating individual factors. Lawmakers could reform sentencing to provide less punitive sentencing options for first- or second-offense defendants, reduce the maximum incarceration period, eliminate three-strikes statutes, or consider other reforms to reduce incarceration.

These reforms carry the potential for public backlash, however. First, lawmakers are accountable to the same punitive public I have referenced throughout this research and face the same desire for re-election. If they are viewed as weak on crime, as some of these reforms could be portrayed by the opposition, they face the prospect of losing their seat in the next election. Second, constraining judicial decision-making runs the risk of creating

an inter-branch conflict, reducing the popularity of the legislature if the judiciary is viewed with great favor, or reducing the credibility of judicial decisions. Politically speaking, such reforms are not without considerable risk to policymakers focused on reelection. The question, then, is whether the long-term gains and savings in fiscal and human capital that are likely to result from a reduction in mass incarceration and racial disparities is worth the short-term political risk politicians are likely to face by being seen as "soft on crime."

Connection to Judicial Elections Arguments

I discussed in chapter 1 some of the arguments for and against judicial elections. Recall that the primary opposition to judicial elections is based on normative grounds from the legal community (e.g., Geyh 2003, 2019), while much of the support for judicial elections comes from empirical studies conducted by political scientists (e.g., Bonneau and Hall 2009; Gibson 2009, 2012; Hall 2001). In this section, I discuss the implications of my findings for both sides of the debate, beginning with the arguments by opponents of judicial elections.

Opponents

Scholars from the legal community who oppose the popular election of judges do so frequently based on a set of normative ideals about how courts *should* operate rather than how they *do* operate. However, some of their claims are testable using data and empirical methods. Although the intent of this research was not to litigate the merits of claims on either side, there are three arguments of election opponents that can be evaluated with the results presented in the preceding chapters.

First, the good news for election opponents. These reformers frequently argue that the independence of judges is hindered by electoral threats. Geyh (2019, 80) articulates this

argument as "judicial elections do not motivate judges to 'call 'em as they see 'em.' Judicial elections motivate judges to call 'em as voters want 'em." The results I presented in the preceding chapters are somewhat mixed on this point. But the evidence strongly suggests that the reformers are not entirely wrong. In many of my analyses, I found that public opinion played a major, often significant, role in conditioning the impact of judicial elections in the criminal justice domain. Election opponents frequently cite other studies (e.g., Canes-Wrone et al. 2014; Gordon and Huber 2007; Huber and Gordon 2004) to argue against electing judges. My research adds to the evidence cited by opponents of judicial elections. Public punitiveness in the states appears to fuel incarceration.

Now for the bad news. Election opponents argue that voters are unable to cast an informed ballot in judicial elections (e.g., Bam 2013; Carlton 2003; Geyh 2003, 2019). To the extent this claim is true, it is also applicable to voters in many other elections, not just judicial elections. That point aside, voters are more likely to cast informed ballots on matters that are particularly salient, such as the issue of crime. And the research I presented in the preceding chapters seems to indicate that judges are not so convinced of the kind of "widespread voter ignorance" that Bam (2013, 565) fears. If judges believed voters were truly ignorant, why would patterns of policy responsiveness emerge? Just as other elected officials fear the electoral vengeance of voters, so do elected judges. Thus, the claim of voter ignorance by judicial reformers appears to miss the point. What is important is not how informed voters are about the candidates, but that judges running for office fear retaliation if they ignore the sentiments of the electorate.

Having questioned the behavior of elected jurists and the competence of the voters who placed them there, election reformers further argue that appointment systems without

subsequent reselection removes politics from the judiciary and permits judges to focus on the rule of law (see, especially, Geyh 2019–82). This does not appear to be consistent with the results presented earlier, which show that supposedly independent judges who do not face the voters are, in many instances, no more or less independent than their elected counterparts.

My results show that judicial elections are far from the whole story. Elections may make a difference at the margins, but here are other factors that appear more important in shaping criminal justice outcomes. Reformers might conclude that to decrease incarceration, there are more important things to focus on, such as sentencing laws and mandatory sentences.

In short, the evidence I present in the preceding chapters is decidedly mixed for election opponents. My results support some of their claims while undermining others. Opponents of judicial elections can find both solace and frustration in my results. But what about election proponents?

Proponents

Supporters of judicial elections can be equally perturbed and excited about my results. Empirical researchers compose the majority of election proponents and base their arguments on rigorous, scientific evidence. Rather than write about how the judiciary *should* work in idealized terms, reach conclusions about how judicial elections actually do work based on empirical observations. They also make claims that are empirically testable, and my results speak to two of their claims.

First, judicial election supporters argue that elections promote accountability (Bonneau and Hall 2009; Hall 2001). Ironically, an election opponent summarizes this

argument best: “The specter of future elections influences judges to exercise their judgment and discretion with reference to the policy preferences of the electorate, instead of their own idiosyncratic ideological biases that would otherwise hold sway” (Geyh 2019, 84). In other words, rather than relying on their own policy preferences the way scholars like Segal and Spaeth (2002) would predict, judges rely on Mayhew (1974) “electoral connection” to translate public preferences into policy (Ginsberg 1976). Thus, as it relates to providing judicial accountability in the criminal justice domain, “judicial elections work as intended” (Geyh 2019, 84).

If judicial elections provide accountability, is one type of election better than another at accomplishing that goal? Many proponents favor partisan elections for a host of reasons. One is that the partisan heuristic conveys meaningful information to voters such that candidates in non-partisan elections must work harder to communicate with voters (Bonneau and Hall 2009, 132). This logic led me to expect differences between states with partisan and non-partisan elections. However, I found little to no evidence to support this contention. On some metrics, election proponents may favor partisan over non-partisan elections, but the evidence I presented in the previous chapters does not lend consistent preference to one or the other scheme.

In sum, election proponents—like election opponents—have reasons to both like and dislike my results. My research supports some of their claims and calls into question others. As a result, as I committed in chapter 1, I will not take a stand on one side of the debate or the other in this research. I leave the wisdom of judicial elections to each individual to decide for herself or himself. My goal has been simply to provide the evidence, even with some limitations.

Limitations and Directions for Future Research

The research presented herein is not perfect. I have contributed to the scholarly debate on mass incarceration, racial disparities, public opinion, judicial elections, representation, and others. My research is not without limitations, however. But I see ample opportunities for future research. In this section, I discuss some of the limitations of this research, the questions it leaves outstanding, and how to address those shortcomings in future scholarship. I additionally discuss future use of the newly created Punitive Attitudes Dataset for the States that I introduced in chapter 3.

The first shortcoming of this research is one that has plagued my thinking since I began the project four years ago. My theory is about judicial behavior—that is, how do judges behave on the bench and why? But all of my empirical analyses are at the state-level rather than the judge-level. Applying an individual-level theory to the state-level can raise legitimate questions of ecological fallacy.

At the same time, however, I carried out the best research possible with the available data. Previous scholarship on trial judges has been able to research judge-level decisions in a single state or locality, raising clear questions of generalizability. My effort has been to explore the dynamic interplay of public opinion, judicial elections, and criminal justice on a broader scale, which required a trade-off between causal inference and generalizability. The state-level, time-series data I assembled permitted me to take a wider view of how public opinion and judicial elections shape criminal justice policy, and I made some decisions that made the ecological problem more palatable, by, for example, excluding states with mixed retention systems. But future research can go further.

Future scholarship on this question must also begin with additional data collection. In the case of trial court judges, waiting for or collecting pristine data can be a serious stumbling block for researchers. One must ask a series of daunting questions. Is the data consistent across states, jurisdictions, courts, and judges? Is it reliable and valid? Excellent data is available on state supreme court justices, but constructing an accurate dataset on trial judges that is as reliable for a trial judge in rural Montana as it is for one in Kansas City, Missouri, or for Los Angeles and New York City is a formidable, time-consuming task. Certainly, high quality research has been done on policy responsiveness at the local level with only larger municipalities (e.g., Tausanovitch and Warshaw 2014), so the task is by no means impossible.

A second limitation deals with the kinds of reforms evaluated. Within the time series contained in dataset, four states changed from partisan to non-partisan elections, permitting an econometric analysis of the consequences of the reform. What I was unable to evaluate is a change from non-electoral to electoral systems or vice versa. Surely, a greater incentive differential exists between elections and non-electoral systems than exists for partisan versus non-partisan elections. To evaluate causal differences, scholars would need either historical data or a contemporary reform. The former would raise questions about applicability to contemporary politics, while the latter seems highly unlikely at this point, given the types of reforms recently proposed and their utter failure to move through the legislative process.

Third, the four reform states studied were alike in many ways. First of all, the states were limited geographically. All four states with reforms—Arkansas, Georgia, Mississippi,

and North Carolina—are southern states. Scholars conducting future research will hopefully have the occasion to investigate policy reforms in non-southern states.

Additionally, all four states are strikingly similar in terms of policy preferences. All four had levels of punitive mood that were well within two standard deviations from the national mean in the year of their reform. Arkansas, in 2001, had a *Public Punitiveness* score .57 (1.73 standard deviation above the national mean). Georgia's score in 1984 was .63 (.05 standard deviation below the mean), Mississippi's 1995 score was .56 (.04 standard deviation below the mean), and in 1996, North Carolina's score was .58 (1.38 standard deviation above the mean).

Despite the limitations discussed above, the contributions of this research have been many. Perhaps the most important contribution is the introduction of the Punitive Attitudes Dataset for the States (PADS). As discussed at length in chapter 3, previous punitive mood data varied either across states or over-time, but never both. PADS varies across states and years and thus provides an excellent resource for future researchers interested in both the antecedents and consequences of public mood as it relates to crime and punishment.

Crime is a salient issue in American politics. But how does it become so? Perhaps a rise in crime brings about a greater call for punitiveness from the mass public, and perhaps a decline in crime precedes a drop in punitive attitudes.

Who (or what) leads whom? As it relates to punitive attitudes, does elite rhetoric follow shifts in public mood or do elites set the tone and the masses follow, as Lenz (2012) might predict? Evaluating elite communication—both official and campaign—using data from PADS can help understand which direction the policy-opinion arrow points.

I have shown that one policy outcome—incarceration rates—is impacted by public opinion. What about other policy in the criminal justice arena? Can punitive attitudes, as captured in PADS, help predict the passage of criminal justice legislation, such as three-strikes laws or bills creating mandatory minimum sentences? Or perhaps declining punitiveness explains repeal of such provisions or the success of other liberalizing criminal justice reform packages. Clearly, data from the PADS is versatile and is of great value to future research.

A Final Thought

Criminal justice in the American states is far from perfect. Government control over citizens through the criminalization of behavior is more expansive than ever before, and the consequences of such government intrusion is evidenced by the explosion of incarceration in recent decades. Tragically, Black Americans bear the brunt of this over-criminalization. Compared with their White compatriots, Blacks are arrested at higher rates, sentenced to longer terms of incarceration, and make up a disproportionate share of prison populations. This is not to mention the well-publicized instances of maltreatment of Black suspects at the hands of a relatively few rogue police officers around the country. Clearly far-reaching criminal justice reform is sorely needed.

But where do representation and judicial elections fit into that reform process? Or should they fit at all? Recent legal discussion has lamented the role of judicial elections in contributing to the mass incarceration problem in the US. But, many states have already limited judicial discretion with sentencing guidelines and other mechanisms. And, if elected judges are simply representing the people, perhaps the task is to change the hearts

and minds of the American people on matters of crime and punishment rather than waging warfare on a democratic institution.

But is it truly a democracy—in a more contemporary sense of the word—without equal protection for all? If defendants in North Carolina or New York are treated differently by elected judges than defendants across state lines in South Carolina or Connecticut, where judges do not face election by the public, some observers would argue that equal protection is lacking.

Federal law, including the constitution, are supreme over state laws and variations in institutional arrangements. But can the supremacy of federal law dictate how a state selects jurists without running afoul of federalism and the 10th amendment? Surely states with long traditions of judicial elections would balk at the federal government mandating a change.

The questions raised are not easily answered. Nor is change, if desired, easily accomplished. However, the questions must be asked, and we must seek answers. If we desire change, we must work for it rather than apathetically expecting it to magically obtain or lazily expecting others to carry our water.

Clearly judicial elections impact criminal justice policy in some ways but not in others. The Reverend Dr. Martin Luther King Jr. once opined: "There comes a time when one must take a position that is neither safe, nor politic, nor popular, but he must take it because conscience tells him it is right." Twice now, I have said I will not attempt to adjudicate between the two sides of the judicial elections debate, and I remain so committed. The ultimate philosophical question I leave is this: what does your conscience tell you is right?

Appendices

Appendix for Chapter 2

Figure A2-1: Histograms for Dependent Variables

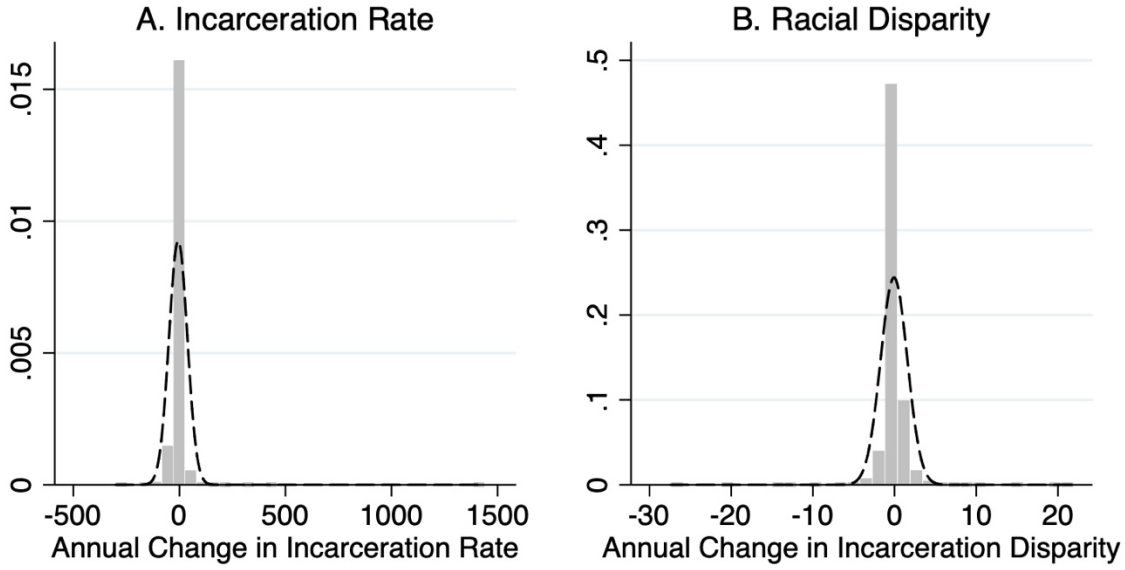


Table A2-1: Descriptive Statistics, Unlogged Dependent Variables

	Mean	Standard Deviation	Min	Max
Dependent Variables				
Δ Incarceration Rate	–5.64	43.08	–300.42	1439.03
Δ Black Disparity	–0.07	1.63	–27.39	21.95

Table A2-2: Correlation Coefficients of Variables

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Δ Incarceration Rate	1.00													
2. Δ Black Disparity	.00	1.00												
3. Retention	–.01	.00	1.00											
4. Public Punitiveness$_{t-3}$	–.03	.04	.22	1.00										
5. Arrest Rate$_{t-1}$	.36	.04	.08	.06	1.00									
6. Arrest Disparity$_{t-1}$	–.02	–.02	–.15	–.03	–.01	1.00								
7. No IAC	–.01	–.02	–.26	–.04	–.14	.06	1.00							
8. Elected Prosecutor	–.00	–.01	.31	.10	–.08	–.04	.14	1.00						
9. Term Length	.01	–.01	–.46	–.12	–.14	.06	.27	–.20	1.00					
10. Recall of Judges	–.01	–.01	.13	–.06	.12	–.08	–.11	.11	–.15	1.00				
11. South	–.02	.03	.29	.22	.24	–.08	–.27	.14	–.18	–.22	1.00			
12. Black Population	.02	.03	.23	.06	.25	–.13	–.46	–.09	–.03	–.16	.57	1.00		
13. Percent Bachelors	.06	–.06	–.29	–.46	.01	–.09	–.00	–.15	.13	.05	–.19	–.04	1.00	
14. State Income	.00	–.01	.01	–.01	.02	–.02	–.02	.01	–.01	.07	–.02	.01	.04	1.00

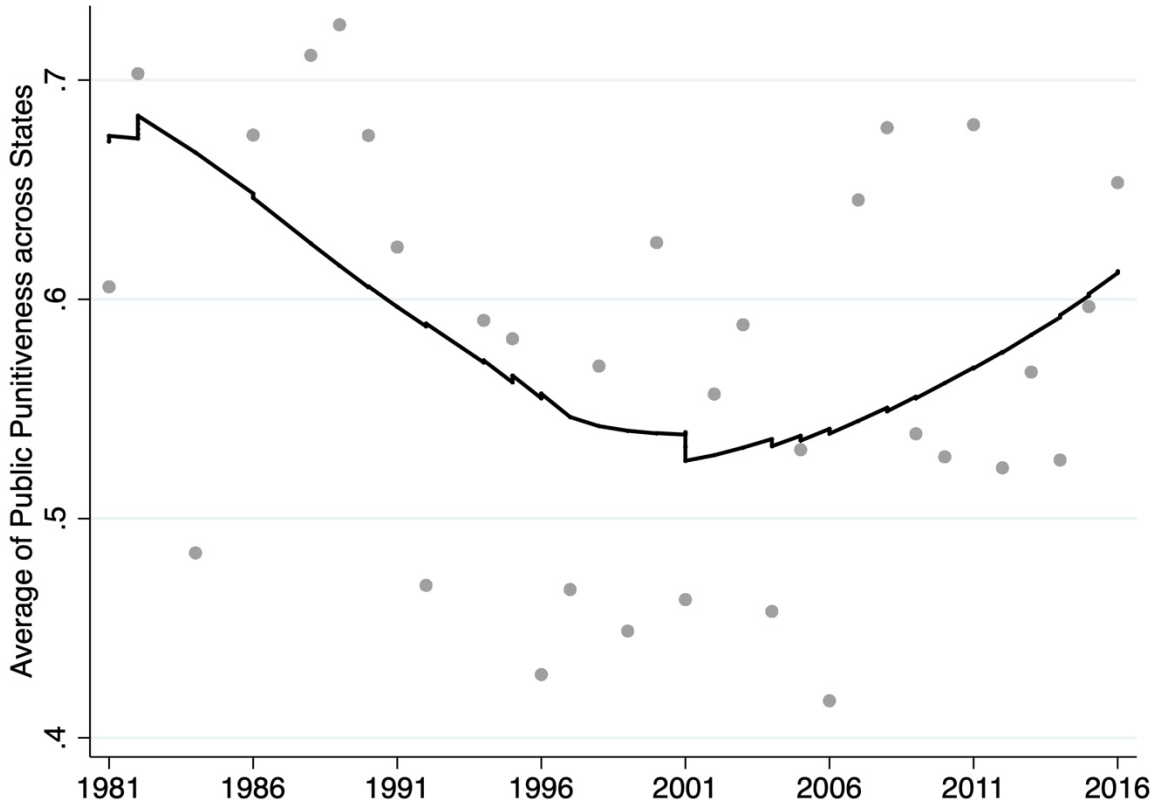
Appendix for Chapter 3

Table A3-1: Survey Items and Question Wording

Survey Item	Question Wording
Cocaine Legalization	Do you favor or oppose legalizing the possession of small amounts of cocaine for personal use?
Favor/Oppose Death Penalty	Do you favor or oppose the death penalty for persons convicted of murder?
Circumstances Death Penalty Justified	Are there any circumstances under which you think the death penalty is justified?
Death Penalty Deters Crime	Do you think capital punishment – the death penalty – is or is not a deterrent to murder?
Death Penalty Deter Terrorism	Please tell me whether you strongly favor, favor, oppose, or strongly oppose the death penalty for persons convicted of terrorism.
Police Hold Suspect Before Charging	If a person is suspected of a serious crime, do you think the police should be allowed to hold him in jail until they can get enough evidence to official charge him?
Treat Juveniles Same as Adults	Do you think that juveniles who are 13 years old who are convicted of a violent crime should be punished in the same way as adults or not?
Try Juveniles in Adult Court	Do you think that juveniles who are 13 years old who are accused of committing a violent crime should be tried in the same court as adult offenders, or in juvenile court?
Courts are Too Lenient	Most judges have more sympathy for the criminals than for their victims.
Penalty for Murder – Life or Death	What do you think should be the penalty for murder – the death penalty or life in prison without any chance of parole?
Penalty for Murder – Prison, Life, or Death	What do you think should be the penalty for murder – the death penalty, life in prison without any chance of parole, or a long prison sentence with a chance of parole?
More Police Reduce Crime	Please tell me whether you think increasing the number of police is a very effective way to reduce crime, a somewhat effective way, a somewhat ineffective way, or a very effective way.

Marijuana Legalization	Do you think that the use of marijuana should be made legal or not?
Legal for Adult to Purchase Marijuana	Do you think it should be legal for an adult to buy marijuana?
Legalize Marijuana for Personal Use	Do you favor or oppose legalizing the possession of small amounts of marijuana for personal use?
Purpose of Prisons	Do you think prisons should be most concerned with rehabilitating convicts, with punishing them, or with keeping them off the streets to protect society?
Shorter Sentences	Please tell me whether you think making criminals serve longer sentences is a very effective way to reduce crime, a somewhat effective way, a somewhat ineffective way, or a very effective way.
How to Solve Crime	What do you think is the best way of reducing crime?
How to Solve Drug Problem	Would a big government push be very effective in controlling drugs, only somewhat effective, or not effective at all
Crime Budget	If you had a say in making up the federal budget this year…should spending on dealing with crime be increased, decreased, or kept about the same?
Reduce Crime Budget	In order to reduce the federal budget deficit, should the government cut spending on anticrime programs or not?
Cuts to Crime Budget	Please tell me if you think Congress is trying to make too many cuts, too few cuts, or about the right amount of cuts to anti-crime programs.
Drug Prevention Budget	Do you think the federal government spending for preventing drug abuse should be increased, decreased, or kept about the same?
Police Search without Warrant	Please tell me how much you agree or disagree that the police should be allowed to search the house of known drug dealers without a court order.

Figure A3-1: Lowess Smoothed Punitive Attitudes Across Time, 1981-2016



Note: This figure displays the Lowess-smoothed average percentage of Americans expressing a punitive opinion in a given year. Bandwidth = 0.8.

Table A3-2: Survey Items, Two Dimensions

Survey Item	Commonality with Index	
	Dimension 1	Dimension 2
Cocaine Legalization	–1.000	–1.000
Favor/Oppose Death Penalty	0.815	0.776
Circumstances Death Penalty Justified	0.993	–0.691
Death Penalty Deters Crime	0.991	0.975
Death Penalty Deter Terrorism	1.000	1.000
Police Hold Suspect Before Charging	1.000	–1.000
Treat Juveniles Same as Adults	–1.000	–1.000
Try Juveniles in Adult Court	1.000	1.000
Courts are Too Lenient	–0.999	–0.924
Penalty for Murder – Life or Death	0.811	0.826
Penalty for Murder – Prison, Life, or Death	–0.550	–0.669
More Police Reduce Crime	1.000	1.000
Marijuana Legalization	1.000	0.985
Legal for Adult to Purchase Marijuana	0.972	0.812
Legalize Marijuana for Personal Use	–1.000	–1.000
Purpose of Prisons	1.000	1.000
Shorter Sentences	1.000	1.000
How to Solve Crime	–1.000	–1.000
How to Solve Drug Problem	–1.000	–1.000
Crime Budget	0.740	0.538
Reduce Crime Budget	–1.000	–1.000
Cuts to Crime Budget	–1.000	–1.000
Drug Prevention Budget	0.979	0.997
Police Search without Warrant	0.824	–0.832
Eigenvalue	1.99	0.33
Variance Explained	81.22%	13.63%

Table A3-3: Predicting a Punitive Response

	Model A3-1 Multilevel Model	Model A3-2 Probit Model
Individual Level (L1)		
Age	–0.000 (0.001)	–0.000 (0.001)
Female	–0.001 (0.003)	–0.001 (0.008)
Education	–0.040 (0.002)***	–0.105 (0.004)***
Income	–0.001 (0.001)	–0.004 (0.002)*
White	0.007 (0.019)	0.026 (0.049)
Black	–0.033 (0.020)*	–0.079 (0.050)
Other Race	0.030 (0.019)	0.086 (0.050)
Hispanic	–0.010 (0.003)***	–0.026 (0.006)***
Democrat	–0.006 (0.004)	–0.017 (0.010)*
Republican	0.075 (0.004) ***	0.194 (0.011)***
State Level (L2)		
Percent Black	0.001 (0.001)***	0.002 (0.001)***
Percent Hispanic	0.001 (0.001)**	0.000 (0.001)
Percent Bachelor's	–0.001 (0.001)*	–0.001 (0.001)
GDP	–0.000 (0.001)***	–0.000 (0.001)***
Crime Rate	–0.000 (0.001)	–0.000 (0.001)
Model Statistics		
Observations	103,257	103,257
Intercept	0.702 (0.025)***	0.486 (0.057)***
State-Level Intercepts	0.001 (0.001)	—
Individual-Level Residual	0.242 (0.001)	—
AIC	146,676.20	140,176.1
BIC	146,848	140,328.8

Table A3-4: Relationship between Public Punitiveness and State Incarceration

	Model 3-1	Model 3-1a	Model 3-1b
Public Punitiveness$_{t-3}$	77.090*	—	—
	(36.480)		
Public Punitiveness$_{t-1}$	—	11.441	—
		(27.173)	
Public Punitiveness$_{t-5}$	—	—	–73.919*
			(35.540)
Δ Incarceration Rate$_{t-1}$	–0.003	–0.011	0.015
	(0.015)	(0.016)	(0.059)
Violent Crime Rate$_{t-1}$	0.007*	–0.007*	–0.008*
	(0.003)	(0.003)	(0.003)
State GDP$_{t-1}$	0.000*	0.000*	0.000
	(0.000)	(0.000)	(0.000)
Top 1% Income Share$_{t-1}$	0.115	0.567*	0.483*
	(0.272)	(0.234)	(0.211)
% African American$_{t-1}$	0.508*	0.118	0.158
	(0.124)	(0.097)	(0.110)
Republican Control$_{t-1}$	–2.726*	–0.081	0.574
	(0.563)	(0.524)	(0.587)
Northeast	8.567*	–0.440	–1.729
	(2.910)	(2.437)	(2.562)
Midwest	13.294*	–1.155	–2.451
	(2.832)	(1.757)	(1.781)
West	11.772*	1.159	0.669
	(2.617)	(2.057)	(2.081)
Constant	–67.405*	–27.220	26.914
	(23.900)	(18.032)	(23.319)
R^2	0.014	0.078	0.097
N	1,370	1,468	1,272
No. of States	49	49	49

Note: The dependent variable is the change in state incarceration rates. Models 3-1, 3-1a, and 3-1b reports linear regression coefficients with panel corrected standard errors in parentheses with different lags of public opinion on punitiveness. * = $p<0.05$; two-tailed test. Only 49 states are included because Nebraska has a non-partisan legislature.

Appendix for Chapter 4

Table A4-1: Margins for Model 4-3

	Margin (SE)
Public Punitiveness = 0.49	
No Elections	7.28 (6.408)
Uncontested Retention Elections	–21.55 (9.349) **
Non-Partisan Elections	–27.63 (14.837) *
Partisan Elections	–6.24 (11.013)
Public Punitiveness = 0.54	
No Elections	–0.80 (3.907)
Uncontested Retention Elections	–16.99 (5.378) **
Non-Partisan Elections	–11.67 (8.716)
Partisan Elections	–1.59 (6.678)
Public Punitiveness = 0.59	
No Elections	–8.87 (3.137) ***
Uncontested Retention Elections	–12.43 (3.481) ***
Non-Partisan Elections	4.29 (4.981)
Partisan Elections	3.06 (4.196)
Public Punitiveness = 0.64	
No Elections	–16.95 (4.980) ***
Uncontested Retention Elections	–7.87 (6.081)
Non-Partisan Elections	20.24 (8.166) **
Partisan Elections	7.71 (6.056)

Note: Margins are significant as follows: *p< .1 **p< .05 ***p< .01

Table A4-2: Contrast of Retention Mechanisms, Model 4-3

	Contrast	Delta-Method SE	*p*-value
Uncontested Retention Elections vs. No Elections			
At Punitiveness = 0.49	–28.834	11.212	0.010
At Punitiveness = 0.54	–16.196	6.794	0.017
At Punitiveness = 0.59	–3.559	5.217	0.488
At Punitiveness = 0.64	9.080	8.130	0.264
Non-Partisan Elections vs. No Elections			
At Punitiveness = 0.49	–34.909	14.642	0.017
At Punitiveness = 0.54	–10.876	8.906	0.222
At Punitiveness = 0.59	13.157	5.591	0.019
At Punitiveness = 0.64	37.190	8.379	0.001
Partisan Elections vs. No Elections			
At Punitiveness = 0.49	–12.521	11.159	0.226
At Punitiveness = 0.54	–0.797	7.200	0.912
At Punitiveness = 0.59	11.928	5.178	0.021
At Punitiveness = 0.64	24.652	7.033	0.001

Table A4-3: Contrast of Retention Mechanisms, NPE vs. PE

	Contrast	Delta-Method SE	*p*-value
Partisan Elections vs. Non-Partisan Elections			
At Punitiveness = 0.49	21.387	20.082	0.287
At Punitiveness = 0.54	10.079	12.382	0.416
At Punitiveness = 0.59	–1.229	7.259	0.866
At Punitiveness = 0.64	–12.538	10.071	0.213

Table A4-4: Legal, Sociodemographic, Political & Full Models of Incarceration Rate

	Legal Model	Socio-demographic Model	Political Model	Full Model
Uncontested Retention Elections (URE)	–115.25*	–32.64	14.51	–113.03
	(60.629)	(35.773)	(48.270)	(74.344)
Non-Partisan Elections (NPE)	–290.58***	–222.03**	–236.97**	–194.94***
	(68.363)	(86.283)	(112.431)	(73.167)
Partisan Elections (PE)	–43.23	–97.55*	–83.72	–74.37
	(64.175)	(50.699)	(66.647)	(86.953)
Public Punitiveness$_{t-3}$	–68.70	–137.83***	–143.96***	–20.10
	(61.895)	(43.271)	(46.750)	(74.599)
URE x Public Punitiveness$_{t-3}$	173.29*	61.03	–21.06	209.83*
	(104.237)	(62.407)	(83.923)	(126.418)
NPE x Public Punitiveness$_{t-3}$	497.29***	389.35***	419.79**	370.27***
	(115.973)	(146.751)	(188.805)	(121.676)
PE x Public Punitiveness$_{t-3}$	70.58	163.75*	140.38	142.22
	(108.295)	(83.591)	(110.341)	(146.283)
Arrest Rate$_{t-1}$	0.02***	—	—	0.03***
	(0.003)			(0.003)
No IAC	0.81	—	—	–20.11***
	(2.731)			(6.032)
Mandatory Minimum Sentencing Categories	–3.52***	—	—	–1.60
	(1.104)			(1.295)
Determinate Sentencing Law	5.95	—	—	4.98
	(4.468)			(4.840)
Sentencing Commission Score (7=Mandatory)	0.66	—	—	0.69
	(0.715)			(0.773)
Crime Rate	–0.01***	—	—	–0.01***
	(0.002)			(0.002)
Juries Sentence	–33.08***	—	—	–19.21***
	(5.852)			(5.900)
South	—	–4.45	—	–14.19
		(8.351)		(9.976)
Percent Black	—	–0.54	—	–20.89***
		(0.887)		(4.026)
Percent Black^2	—	0.97	—	6.26***
		(0.862)		(1.611)
Bachelors' Degrees	—	3.53	—	–7.24
		(6.500)		(7.191)
State Income$_{t-1}$	—	0.00	—	–0.00
		(0.001)		(0.001)
Population Aged 18–24	—	0.62	—	–0.10
		(0.433)		(0.893)
Percent Evangelical	—	–0.14**	—	–0.51***
		(0.063)		(0.174)
Elected Prosecutors	—	—	8.86	34.74***
			(6.102)	(8.074)
Judicial Term Length	—	—	0.02	0.25***
			(0.041)	(0.091)

Judicial Recall	—	—	–7.67 (7.788)	–40.25*** (8.385)
Democratic Presidential Vote Share	—	—	–0.01 (0.319)	0.06 (0.304)
Democratic Control in State Government	—	—	–2.76 (3.376)	–3.41 (2.746)
Citizen Liberalism	—	—	0.07 (0.134)	0.22 (0.177)
State Government Liberalism	—	—	0.08 (0.208)	0.23 (0.171)
Divided Government	—	—	0.99 (3.554)	1.55 (3.292)
Legislative Professionalism Score	—	—	44.33 (50.240)	29.71 (49.087)
Constant	–21.16 (38.996)	61.19 (39.658)	58.25* (33.656)	–97.01 (63.396)
N	1,253	1,286	1,168	1,137
R^2	0.324	0.021	0.026	0.420
Number of States	46	46	45	45

All models estimated using Stata 15.0's xtpcse command. Data from National Prisoner Statistics, 1978-2015, US Department of Justice, ICPSR Study 36657. DV is the yearly change in a state's incarceration rate per 100,000 population. Correlated panel-corrected standard errors in parentheses. Results of two-tailed tests of significance are as follows: *p< .1 **p< .05 ***p< .01. Autocorrelation corrected using panel-specific AR(1). Unit of analysis is the state-year.

Table A4-5: Antecedents of State Incarceration Rates, Alternative Models

	PCSE	GLS	FLGS
Uncontested Retention Elections (URE)	–152.69**	–66.19	–173.81**
	(63.745)	(69.71)	(78.52)
Non-Partisan Elections (NPE)	–270.43***	–142.33*	–263.14***
	(78.376)	(86.40)	(62.55)
Partisan Elections (PE)	–138.22**	–42.49	–137.26
	(57.963)	(66.88)	(95.27)
Public Punitiveness$_{t-3}$	–161.51**	–128.31**	–157.07**
	(63.838)	(53.60)	(78.69)
URE x Public Punitiveness$_{t-3}$	252.76**	104.64	294.48**
	(109.250)	(113.34)	(136.53)
NPE x Public Punitiveness$_{t-3}$	480.66***	246.57*	477.29***
	(131.919)	(148.04)	(107.78)
PE x Public Punitiveness$_{t-3}$	254.49***	74.49	255.76
	(97.643)	(119.67)	(160.34)
Arrest Rate$_{t-1}$	0.02***	0.01	0.02***
	(0.003)	(0.01)	(0.001)
No Intermediate Appellate Court	–7.48	–7.73	–9.76**
	(4.775)	(8.97)	(4.71)
Elected Prosecutors	24.31***	18.26	24.50***
	(7.543)	(12.85)	(6.09)
Judicial Term Length	0.20***	0.11	0.24***
	(0.062)	(0.09)	(0.08)
Judicial Recall	–35.34***	–11.93	–35.11***
	(8.771)	(11.83)	(6.24)
South	–31.82**	–20.55	–34.02***
	(12.664)	(20.33)	(7.53)
Percent Black	–12.09***	–6.77	–10.43***
	(3.166)	(6.42)	(3.16)
Percent Black^2	4.56***	2.41	4.29***
	(1.171)	(2.36)	(1.40)
Bachelors' Degrees	1.43	7.37	7.10
	(6.600)	(6.42)	(54.67)
State Income$_{t-1}$	0.00	–0.00	–0.00
	(0.001)	(0.00)	(0.00)
Constant	–31.22	–14.31	–54.94
	(49.802)	(46.46)	(55.24)
Wald X^2	100.88***	30.32**	648.03***
N	1,253	1,253	1,253
R^2	0.345	0.17	—
Number of States	46	46	46

PCSE model estimated using Stata 15.0's xtpcse command. GLS model estimated using Stata 15.0's xtreg command. FGLS model estimated using Stata 15.0's xtgls command.

Table A4-6: Synthetic Matching Predictor Balance, Incarceration Rate

	Treated State	Synthetic State
Arkansas[1]		
Incarceration Rate 1978	4.77	4.81
Incarceration Rate Mid-PreTx	5.63	5.66
Incarceration Rate Year PreTx	6.03	5.95
Citizen Liberalism	41.42	38.57
Percent Black	16.39	17.45
Income Per Capita	13.79	15.20
Percent Bachelors' Degree	11.63	14.03
Crime Rate	4343.89	4294.13
Arrest Rate	6195.70	4139.30
Public Punitiveness	0.62	0.60
Georgia[2]		
Incarceration Rate 1978	5.41	5.40
Incarceration Rate Mid-PreTx	5.42	5.42
Incarceration Rate Year PreTx	5.59	5.59
Citizen Liberalism	24.92	32.39
Percent Black	26.35	23.43
Income Per Capita	8.86	9.09
Percent Bachelors' Degree	11.90	12.38
Crime Rate	5190.00	5242.35
Arrest Rate	5336.70	4876.75
Public Punitiveness	.64	.62
Mississippi[3]		
Incarceration Rate 1978	4.41	4.54
Incarceration Rate Mid-PreTx	5.54	5.46
Incarceration Rate Year PreTx	5.90	5.86
Citizen Liberalism	28.01	40.08
Percent Black	35.58	16.93
Income Per Capita	10.87	14.07
Percent Bachelors' Degree	12.27	14.81
Crime Rate	3645.12	4883.27
Arrest Rate	2269.71	4041.08
Public Punitiveness	0.60	0.61
North Carolina[4]		
Incarceration Rate 1978	5.48	5.41
Incarceration Rate Mid-PreTx	5.60	5.52
Incarceration Rate Year PreTx	5.94	6.04
Citizen Liberalism	35.43	38.70
Percent Black	22.26	14.12
Income Per Capita	13.77	15.84
Percent Bachelors' Degree	13.58	15.22

Crime Rate	4849.47	7636.57
Arrest Rate	6175.04	4771.38
Public Punitiveness	0.62	0.63

[1] Matched on AL (0.325); GA (0.217); KY (0.322); MO (0.082); and WV (0.055)
[2] Matched on DE (0.14); MI (0.024); NC (0.184); SC (0.462); TX (0.111); VA (0.079)
[3] Matched on AL (0.616); HI (0.157); KS (0.154); NH (0.073)
[4] Matched on FL (0.889); TN (0.111)

Appendix for Chapter 5

Table A5-1: Ordering of State Black Disparity over Time

1980	1985	1990	1995	2000	2005	2010	2015
ND	AK	ND	HI	HI	HI	KY	HI
SC	ND	HI	VT	GA	GA	HI	NH
VT	SC	AK	ID	ID	MS	MS	ID
MS	HI	TN	AK	MS	AL	GA	WV
HI	GA	SC	MS	AL	AK	AL	KY
GA	MS	NM	AZ	AR	AR	AK	AK
AK	NC	MS	GA	MT	TN	WV	MT
NC	AL	AR	SC	NV	ID	AR	MS
ME	TN	NY	TN	TN	LA	TN	WY
TN	TX	ME	NM	SC	WV	LA	AL
SD	NJ	RI	TX	IL	PA	PA	NE
MD	RI	PA	UT	NY	IL	IL	NM
NJ	PA	MA	NE	UT	CA	CT	NY
PA	NE	WI	NJ	PA	CT	MN	PA
MA	WI	NE	IL	IO	NM	NM	CT
NE	UT	UT	PA	CT	MN	CA	IO
IO	CT	IO	WI	NM	WI	VT	MN
UT	IO	CT	CT	WI	IO	IO	IL
WI	MN	MN	IO	NJ	VT	WI	NJ
MN	VT	VT	MN	MN	NJ	NJ	CA

Table reports the states with the 10 lowest and 10 highest values on Black Disparity for the years listed. Reading the table from top to bottom is associated with increasing values of Black Disparity.

Table A5-2: Margins for Model 5-3

	Margin (SE)
Public Punitiveness = 0.49	
No Elections	–0.25 (0.357)
Uncontested Retention Elections	–0.19 (0.410)
Non-Partisan Elections	–0.07 (0.187)
Partisan Elections	–0.51 (0.222) *
Public Punitiveness = 0.54	
No Elections	–0.11 (0.173)
Uncontested Retention Elections	–0.12 (0.233)
Non-Partisan Elections	–0.12 (0.101)
Partisan Elections	–0.37 (0.121) *
Public Punitiveness = 0.59	
No Elections	0.02 (0.106)
Uncontested Retention Elections	–0.05 (0.127)
Non-Partisan Elections	–0.16 (0.063) *
Partisan Elections	–0.24 (0.073) *
Public Punitiveness = 0.64	
No Elections	0.15 (0.267)
Uncontested Retention Elections	0.02 (0.232)
Non-Partisan Elections	–0.20 (0.129)
Partisan Elections	–0.10 (0.146)

Note: * = Margins significant at $p<0.05$

Table A5-3: Contrast of Retention Mechanisms, Black Disparity

	Contrast	Delta-Method SE	*p*-value
Uncontested Retention			
Elections vs. No Elections			
At Public Punitiveness = 0.49	0.062	0.504	0.903
At Public Punitiveness = 0.54	–0.003	0282	0.991
At Public Punitiveness = 0.59	–0.068	0.181	0.707
At Public Punitiveness = 0.64	–0.133	0.336	0.693
Non-Partisan Elections			
vs. No Elections			
At Public Punitiveness = 0.49	0.172	0.394	0.662
At Public Punitiveness = 0.54	–0.004	0.207	0.985
At Public Punitiveness = 0.59	–0.180	0.135	0.182
At Public Punitiveness = 0.64	–0.355	0.285	0.212
Partisan Elections			
vs. No Elections			
At Public Punitiveness = 0.49	–0.261	0.434	0.547
At Public Punitiveness = 0.54	–0.259	0.219	0.237
At Public Punitiveness = 0.59	–0.257	0.125	0.041
At Public Punitiveness = 0.64	–0.254	0.302	0.400

Table A5-4: Contrast of Retention Mechanisms, Black Disparity

	Contrast	Delta-Method SE	*p*-value
Non-Partisan Elections vs. Partisan Elections			
At Public Punitiveness = 0.49	–0.433	0.278	0.119
At Public Punitiveness = 0.54	–0.255	0.149	0.087
At Public Punitiveness = 0.59	–0.077	0.081	0.340
At Public Punitiveness = 0.64	0.101	0.174	0.561

Table A5-5: Legal, Sociodemographic, Political & Full Models of Black Disparity

	Legal Model	Socio-demographic Model	Political Model	Full Model
Uncontested Retention Elections (URE)	0.27 (2.896)	0.70 (2.940)	0.27 (3.320)	–0.94 (3.330)
Non-Partisan Elections (NPE)	1.94 (2.415)	1.53 (2.313)	1.26 (2.552)	1.36 (2.758)
Partisan Elections (PE)	–0.04 (2.791)	–0.92 (2.615)	–1.46 (3.068)	–2.80 (3.416)
Public Punitiveness$_{t-3}$	2.38 (3.930)	2.88 (3.821)	1.84 (4.361)	–0.77 (4.662)
URE x Public Punitiveness$_{t-3}$	–0.57 (4.955)	–1.14 (5.002)	–0.73 (5.690)	1.52 (5.694)
NPE x Public Punitiveness$_{t-3}$	–3.47 (4.145)	–2.80 (3.986)	–2.50 (4.393)	–2.63 (4.749)
PE x Public Punitiveness$_{t-3}$	–0.06 (4.774)	1.18 (4.492)	2.19 (5.257)	4.31 (5.834)
Δ Black Disparity$_{t-1}$	–0.25*** (0.077)	–0.25*** (0.077)	–0.26*** (0.081)	–0.26*** (0.082)
Arrest Disparity$_{t-1}$	–0.04* (0.023)	—	—	–0.05** (0.025)
No IAC	–0.02 (0.165)	—	—	–0.07 (0.194)
Mandatory Minimum Sentencing Categories	0.01 (0.048)	—	—	0.02 (0.037)
Determinate Sentencing Law	0.00 (0.121)	—	—	–0.13 (0.134)
Sentencing Commission Score (7=Mandatory)	–0.03* (0.017)	—	—	–0.02 (0.018)
Crime Rate	0.00** (0.000)	—	—	0.00 (0.000)
Juries Sentence	0.04 (0.117)	—	—	0.10 (0.117)
South	—	0.20 (0.149)	—	0.10 (0.160)
Percent Black	—	–0.01 (0.058)	—	0.00 (0.068)
Bachelors' Degrees	—	–0.33** (0.137)	—	–0.26 (0.170)
State Income$_{t-1}$	—	0.00 (0.000)	—	0.00 (0.000)
Population Aged 18–24	—	0.01 (0.008)	—	0.01 (0.010)
Percent Evangelical	—	–0.00 (0.003)	—	–0.01* (0.006)
Elected Prosecutors	—	—	0.13 (0.141)	0.15 (0.174)
Judicial Term Length	—	—	–0.00 (0.006)	–0.00 (0.005)

Judicial Recall	—	—	–0.11 (0.136)	–0.12 (0.139)
Democratic Presidential Vote Share	—	—	–0.02** (0.010)	–0.02* (0.012)
Democratic Control in State Government	—	—	0.04 (0.084)	0.02 (0.091)
Citizen Liberalism	—	—	0.00 (0.005)	0.00 (0.005)
State Government Liberalism	—	—	0.00 (0.004)	0.00 (0.005)
Divided Government	—	—	0.02 (0.094)	0.01 (0.099)
Legislative Professionalism Score	—	—	1.07** (0.517)	0.26 (0.707)
Constant	–1.70 (2.259)	–0.72 (2.299)	–0.71 (2.600)	1.76 (2.877)
N	1,219	1,269	1,153	1,105
R^2	0.079	0.075	0.079	0.093
Number of States	46	46	45	45

Models estimated using Stata 15.0's xtpcse command. Data from National Prisoner Statistics, 1978-2015, US Department of Justice, ICPSR Study 36657. DV is yearly change in a state's Black-to-White incarceration disparity rate. Panel-corrected standard errors in parentheses. Results of two-tailed tests of significance are: *p< .1 **p< .05 ***p< .01. Autocorrelation corrected using panel-specific AR(1). Unit of analysis is the state-year.

Figure A5-1: Predicting Racial Disparity in Prison Population by Selection of Judges across Racial Punitiveness, States with either Partisan or Non-Partisan Elections; Baseline Changed to Include Uncontested Retention Elections

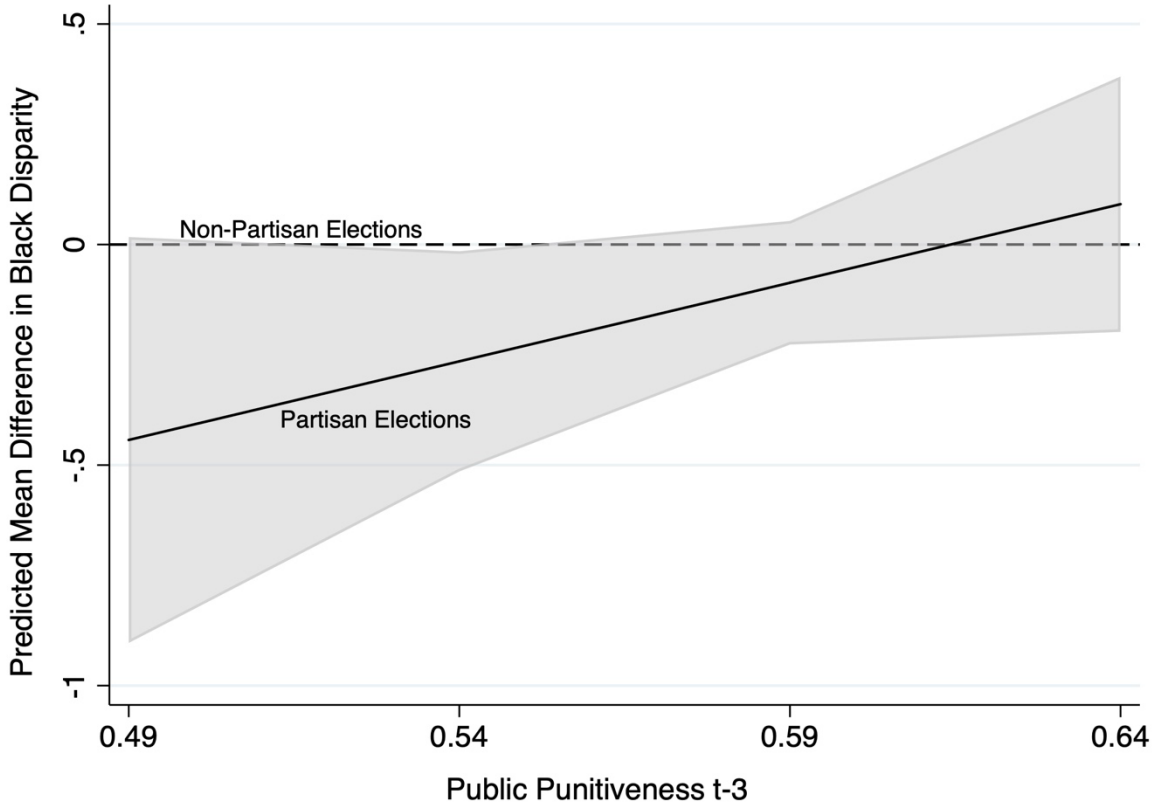


Table A5-6: Predicting Racial Incarceration Disparities based on Judicial Retention Mechanisms and Public Opinion, Alternative Models

	PCSE	GLS	FLGS
Reference Category is No Elections			
Uncontested Retention Elections (URE)	0.70 (2.949)	1.86 (1.46)	0.92 (2.33)
Non-Partisan Elections (NPE)	1.89 (2.429)	2.17* (1.14)	1.84 (1.71)
Partisan Elections (PE)	–0.28 (2.695)	1.71 (1.46)	–0.32 (2.27)
Public Punitiveness$_{t-3}$	2.68 (3.966)	3.78*** (1.38)	2.79 (2.30)
Δ Black Disparity$_{t-1}$	–0.25*** (0.077)	–0.30*** (0.08)	–0.26*** (0.03)
URE x Public Punitiveness$_{t-3}$	–1.30 (5.044)	–3.36 (2.49)	–1.70 (4.03)
NPE x Public Punitiveness$_{t-3}$	–3.52 (4.190)	–4.04** (1.95)	–3.44 (2.96)
PE x Public Punitiveness$_{t-3}$	0.05 (4.642)	–3.25 (2.41)	0.10 (3.85)
Arrest Rate Disparity$_{t-1}$	–0.05** (0.022)	–0.03** (0.01)	–0.04** (0.02)
No Intermediate Appellate Court	0.03 (0.228)	–0.00 (0.06)	0.01 (0.12)
Elected Prosecutors	–0.12 (0.096)	–0.10 (0.09)	–0.12 (0.18)
Judicial Term Length	–0.00 (0.005)	–0.00 0.00)	–0.00 (0.00)
Judicial Recall	–0.04 (0.121)	0.02 (0.11)	–0.02 (0.13)
South	0.08 (0.112)	0.04 (0.06)	0.08 (0.16)
Black Population	0.03 (0.050)	0.02 (0.02)	0.02 (0.04)
Bachelor's Degrees	–0.36** (0.141)	–0.35*** (0.07)	–0.34** (0.15)
State Income$_{t-1}$	–0.00 (0.000)	–0.00*** (0.001)	–0.00 (0.00)
Constant	–0.21 (2.350)	–0.94 (0.96)	–0.32 (1.56)
Wald X^2	36.13***	416.15***	98.46***
N	1,219	1,219	1,219
R^2	0.075	0.095	—
Number of States	46	46	46

PCSE model estimated using Stata 15.0's xtpcse command. GLS model estimated using Stata 15.0's xtreg command. FGLS model estimated using Stata 15.0's xtgls command.

Table A5-7: Predicting Incarceration Rates based on Judicial Retention Mechanisms and Public Opinion, by Race

	White Incarceration	Black Incarceration
Uncontested Retention Elections (URE)	27.31	-10.55
	(65.223)	(374.241)
Non-Partisan Elections (NPE)	-21.08	188.52
	(54.215)	(246.251)
Partisan Elections (PE)	72.23	443.92
	(133.261)	(274.281)
Public Punitiveness$_{t-3}$	33.70	643.15
	(50.145)	(537.689)
URE x Public Punitiveness$_{t-3}$	-48.77	-49.04
	(111.375)	(643.103)
NPE x Public Punitiveness$_{t-3}$	38.10	-400.37
	(89.496)	(416.827)
PE x Public Punitiveness$_{t-3}$	-120.07	-827.46*
	(230.723)	(467.705)
Δ Racial Incarceration Rate$_{t-1}$	-0.36***	0.01
	(0.136)	(0.018)
Racial Arrest Rate$_{t-1}$	0.00	-0.00
	(0.001)	(0.001)
No Intermediate Appellate Court	-0.31	-18.95
	(2.331)	(14.113)
Elected Prosecutors	-3.93	-23.66
	(13.880)	(32.301)
Judicial Term Length	-0.03	-0.36
	(0.135)	(0.469)
Judicial Recall	-4.13	8.83
	(4.622)	(13.150)
South	1.54	3.83
	(14.983)	(15.839)
Black Population	-0.94	-4.00
	(0.890)	(10.311)
Bachelor's Degrees	-13.90	-132.78***
	(9.299)	(36.334)
State Income$_{t-1}$	0.00	-0.00
	(0.000)	(0.006)
Constant	32.51	123.51
	(54.700)	(343.333)
N	1,239	1,223
R^2	0.141	0.055
Number of States	46	46

Models estimated using Stata 15.0's xtpcse command. Data from National Prisoner Statistics, 1978-2015, US Department of Justice, ICPSR Study 36657. DV is yearly change in a state's incarceration rate by race. Panel-corrected standard errors in parentheses. Results of two-tailed tests of significance are: *p< .1 **p< .05 ***p< .01. Autocorrelation corrected using panel-specific AR(1). Unit of analysis is the state-year.

Figure A5-2: Predicting Incarceration Rates based on Judicial Retention Mechanisms and Public Opinion, by Race

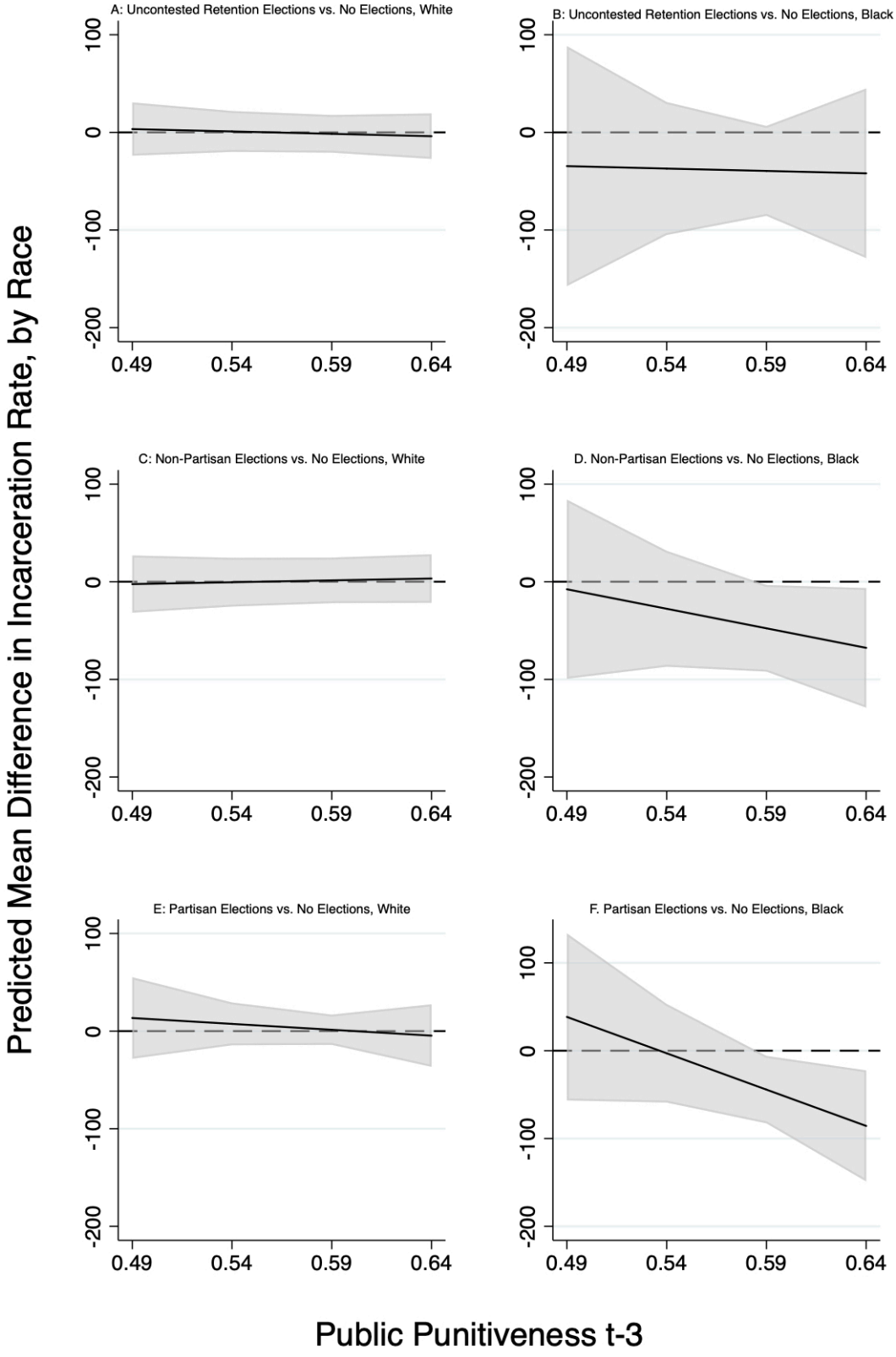


Table A5-8: Synthetic Matching Predictor Balance, Black Disparity

	Treated State	Synthetic State
Arkansas[1]		
Black Disparity 1978	5.16	5.78
Black Disparity Mid-PreTx	4.56	5.71
Black Disparity Year PreTx	5.35	6.14
Citizen Liberalism	41.42	41.44
Percent Black	16.39	16.20
Income Per Capita	13.79	16.47
Percent Bachelors Degree	11.63	13.29
Crime Rate	4343.89	4343.49
Arrest Disparity	3.06	3.06
Racial Punitiveness	0.09	0.09
Mississippi[2]		
Black Disparity 1978	3.61	3.58
Black Disparity Mid-PreTx	4.24	4.09
Black Disparity Year PreTx	5.29	5.49
Citizen Liberalism	28.58	39.02
Percent Black	35.58	22.63
Income Per Capita	10.53	12.37
Percent Bachelors Degree	12.12	13.34
Crime Rate	3593.98	4862.69
Arrest Disparity	4.84	4.21
Racial Punitiveness	0.06	0.07
North Carolina[3]		
Black Disparity 1978	4.18	3.99
Black Disparity Mid-PreTx	4.40	4.56
Black Disparity Year PreTx	7.10	7.01
Citizen Liberalism	35.43	38.54
Percent Black	22.26	20.70
Income Per Capita	13.77	13.42
Percent Bachelors Degree	13.58	14.04
Crime Rate	4849.47	5108.20
Arrest Disparity	2.77	4.08
Racial Punitiveness	0.08	0.08

[1] Matched on AL (0.288); IN (0.252); LA (0.078); MS (0.070); NV (0.075); SC (0.031); WV (0.206)

[2] Matched on ID (0.088); KY (0.143); NY (0.016); ND (0.462); SC (0.705)

[3] Matched on AL (0.123); IN (0.151); NH (0.050); ND (0.054); SC (0.487); TX (0.135)

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Vita

Travis Nathan Taylor

EDUCATION

M.A. University of Kentucky, Political Science, August 2019
M.P.S. The George Washington University, Political Management, May 2014
B.A. University of Louisiana at Lafayette, Political Science, December 2010

ACADEMIC POSITIONS

University of Kentucky, Department of Political Science
2019 – 2020 *Instructor of Political Science*
2016 – 2018 *Teaching Assistant*

New York University, School of Continuing and Professional Services
2015 – 2016 *Instructor of Political Management*

The George Washington University, Graduate School of Political Management
Fall 2014 *Facilitator of Political Management*

PUBLICATIONS

Peffley, Mark, Alexander Denison, and **Travis N. Taylor**. 2020. "Print, Electronic, and Social Media and the Transformation of Democratic Representation." In *Handbook of Political Representation in Liberal Democracies*, ed. R. Rohrschneider and J. Thomassen. New York: Oxford University Press.

ACCOMPLISHMENTS, AWARDS, AND HONORS

Graduate School

- Crum Emerging Scholar Award, Dept. of Political Science, Univ. of Kentucky (2020)
- Alumni Development Fund Research Award, Dept. of Political Science, Univ. of Kentucky, $500 (2020)
- Best Graduate Student Paper, Kentucky Political Science Association (2020)
- Carsey Scholar Award, State Politics & Policy Section, American Political Science Association, $300 (2020)
- Crum Emerging Scholar Award, Dept. of Political Science, Univ. of Kentucky (2019)
- Ken and Mary Sue Coleman Award, Dept. of Political Science, Univ. of Kentucky, $1445 (2018)
- Political Science Alumni Endowed Doctoral Research and Travel Award, Dept. of Political Science, Univ. of Kentucky, $280 (2018)
- Graduate Student Best Paper Nomination, Kentucky Political Science Association (2018)
- APSA Minority Fellowship, American Political Science Association, $500 (2017–18)
- Lyman T. Johnson Minority Fellowship, The Graduate School, University of

Kentucky (2016–19)
- Teaching Assistantship, Department of Political Science, University of Kentucky (2016–20)
- Second Service Fellowship, The George Washington University (2012–14)

Professional
- 40 Under 40 Award Nomination, American Association of Political Consultants (AAPC) (2016)
- Bronze Pollie Award for Best Use of TV-Student, AAPC (2014)
- Silver Pollie Award for Best Use of Social Pressure, AAPC (2013)
- Bronze Pollie Award for Best Use of Social Pressure, AAPC (2013)
- Meritorious Service Award (2nd), Acadian Ambulance Service (2006)
- Meritorious Service Award, Acadian Ambulance Service (2006)
- Air Assault Badge, U.S. Army (2005)
- Global War on Terrorism Service Medal (2004)
- Non-Commissioned Officer Professional Development Ribbon, U.S. Army (2004)
- Army Reserve Component's Achievement Medal, U.S. Army (2003)
- Army Achievement Medal, U.S. Army (2002)

Other Academic
- Featured Alumni, University of Louisiana at Lafayette (2016)
- Institute of Politics Fellowship, Loyola University New Orleans (2015–16)
- Political Science Distinguished Graduate Finalist, University of Louisiana at Lafayette (2010)
- Founded the University of Louisiana at Lafayette Political Science Club (2010)
- University of Louisiana at Lafayette Foundation Scholarship (2010)
- Award of Merit, University of Louisiana at Lafayette (2010)
- Inducted into The Honor Society of Phi Kappa Phi (2010)
- President's List, University of Louisiana at Lafayette (2009–10)
- University of Louisiana at Lafayette Transfer Scholarship (2009–10)
- Cherokee Nation Higher Education Scholarship (2009–10)
- Chancellor's List, South Louisiana Community College (2008)
- Dean's List, South Louisiana Community College (2006)